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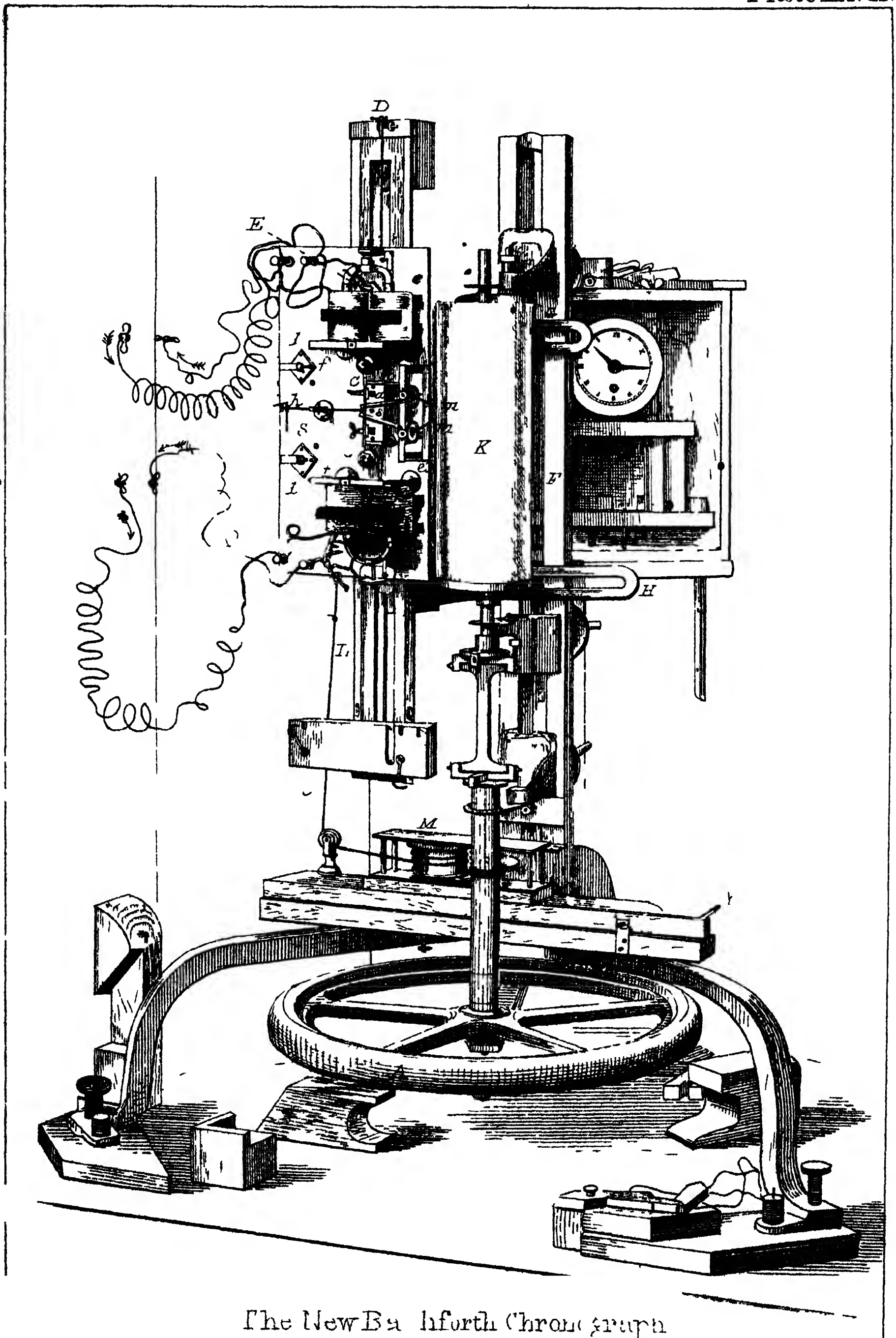



The New Bashforth Chronograph

POPULAR SCIENCE REVIEW.

# HITTING THE MARK; OR, CANNON-BALLS AND THEIR STRIKING VELOCITY.

BY G. WEST ROYSTON-PIGOTT, M.A., M.D., CANTAB. M.R.C.P.,

LATE FELLOW OF ST. PETER'S COLLEGE, CAMBRIDGE, AND FELLOW OF THE CAMBRIDGE PHILOSOPHICAL, THE ROYAL ASTRONOMICAL, AND MICROSCOPICAL SOCIETIES, AUTHOR OF "THE HARROGATE SPAS."

[PLATE LXVII.]

EVERY few years, we are now accustomed to hear of campaigns on a most stupendous scale being fought, lost or won, in a great measure by superiority of modern artillery. The nation which has the best field-gun and can strike the most rapid, overwhelming, and hard-hitting blows at long range, demoralises the foe before he has a chance to hit again. Electricity for conveying orders, and explosive missiles despatched at fabulous distances, enable concentric masses to annihilate the enemy, innocent of such resources of the modern art of killing the greatest number in the least possible time.

SOLFERINO, SADOWA, and SEDAN hissed a sad sarcasm, from the vainglorious vanquished, whilst Europe rang with sympathetic echoes, trembling at the fall of embattled hosts, destroyed for lack of knowledge. Yet the philosophy of projectiles seems only just now beginning to be understood. A practical working knowledge of the effective energy of shot hitting a distant mark is of more importance now than ever. In face of the European dramas so recently played out before our eyes, the question of striking energy, or distant velocity, is now of supreme, it may be hereafter of tremendous interest, as involving the very destinies of the country.

Here is a question of apparent simplicity which our artillerists could not recently solve.

If a 9-inch 250 lb. cannon-ball strikes an object 200 feet distant with a velocity of 1,400 feet per second (1,400 f. s.),

what loss of velocity would be effected by the resistance of the air upon its reaching an iron-clad vessel a mile distant?

To fire cannon-balls with a given charge and ascertain their time of flight and range, or perhaps their penetrating power at short ranges; and to "time" the fuze of a bursting charge, were the chief points formerly attended to.

Attempts had been made, indeed, with considerable success, to ascertain one velocity for each ball striking a ballistic pendulum, which, upon receiving the shock of the striking ball, vibrated through a measurable arc.

Even this solution was sufficiently difficult to have engaged the powers of *Robins*, *Hutton*, *Didion*, and *Helié*. So late as 1865 the latter confesses ("Traité de Balistique"): "*Les solutions les plus avancées laissent encore fort à désirer. L'exposé de ce qui a été fait montrera du moins ce qu'il reste à faire*" (p. 2).

There still remains, however, a great *desideratum*—a complete system which shall enable the scientific artillerist to answer any question respecting *the motion and behaviour of a shot after it has left the cannon's mouth at any point of its path.* Such results should also be immediately practicable by those who are without the scientific knowledge on which such results have been obtained. "Applied science," the master idea of the age, would, in the case of artillery students, receive a striking illustration: and thus literally connect the Government with technical education. The general reader will understand the remarkable difficulties encountered in prosecuting this research, by presenting to him a short *résumé* of the history of this engrossing question.

Nearly the whole system of theoretic modern gunnery is founded on Hutton's "Mathematical Course" (H. died 1807). Hutton's celebrated experiments with the ballistic pendulum were published by the Royal Society in their Transactions in 1778, nearly one hundred years ago: these experiments had an important influence.*

* Several European Governments took up the question. The chief theatre of them was Metz in 1839–40, where an extensive system was developed under the direction of MM. *Piobert*, *Morin*, and *Didion*, with a large instrument constructed on the English plan. In 1855, a large ballistic pendulum was constructed at Elswick for the English government, at the cost of several thousand pounds, which has not hitherto been even used, for shortly afterwards Navez's electro-ballistic instrument was imported from Belgium, which appeared to give correct initial velocities. But when the mathematicians began to cross-question its results, they found that no law of resistance of the air at various points of the ball's path could be deduced from its use. Since 1866 Benton's electro-ballistic machine with two pen-

In a languid melancholy way we have been arriving (for a reward of 200 years' study of this question) at an inkling of the real state of the case. In gunnery our parabolic theory was all wrong. It was fancied, somehow, first of all that the resistance of the air had little power as against shot. Sir Isaac Newton and Dr. Halley were both of this opinion. Robins states ("Gunnery," Preface), that a musket-ball at an elevation of 45° should range seventeen miles in vacuo, yet it only flies half a mile! *

The great honour of mastering the complicated difficulties attending the determination of successive velocities at different points of the flight, and calculating the laws of the resistance of the air from experiments made upon a large scale, is unquestionably due to our fellow-countryman, Professor Bashforth. This gentleman, apparently from the tenour of the Report, received little approval or encouragement from the official mind.

The English public will no doubt appreciate these labours, and it is a matter of congratulation that such men as Professors Adams and Stokes have undertaken the onerous task of pronouncing their verdict upon researches as profound as they were spontaneous. An elaborate Report is now published replete with results of the highest importance to the future defensive power of this country.† Quoting from this report we read:—

dulums, under the name of Leur's, and another by Boulengé, which give only one velocity, have been much used in this country and on the Continent. Sir Charles Wheatstone, F.R.S., and Breguet, also designed instruments for measuring successive velocities, but no information is extant upon their experimental success. At Paris, in 1867, Schultz's instrument was also exhibited with a similar intention and *no* result.

* The resistance of the air in the case just mentioned reduces the range to a space thirty-four times less than a vacuum range.

Hutton's law assigned it to be a function of the velocity added to its square ($av + bv^2$).

Didion at a function of the velocity added to the cube ($bv + cv^3$).

Colonel Mayewski at a higher function still—

$$bv^2 + dv^4.$$

What is very remarkable is that Hutton and Piobert (whose laws have been commonly employed) deduced very different laws from the same experiments.

For further information on this point see "*Description of a Chronograph, adapted for Measuring the varying Velocity of a Body in Motion through the Air, and other Purposes.*" London: Bell & Daldy.

† *Report on Experiments made with the Bashforth Chronograph for determining the Resistance of the Air to the Motion of Projectiles* (p. 169). Printed by Eyre & Spottiswoode, November 1870, for Her Majesty's Stationery Office.

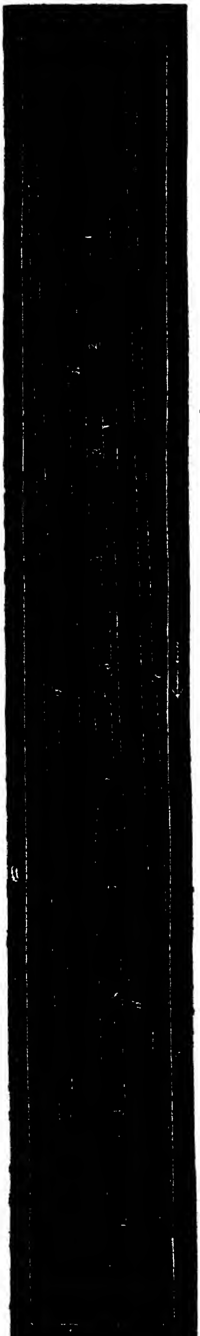


Fig. 1.

*a, b, c, d, e* denote the records made by the clock every second.
1, 2, 3, 4, 5, 6, 7, 8, 9, 10 denote the records obtained from the screens placed at intervals of 150 feet in the path of the projectile.

(March 21, 1864.)—"Soon after his appointment, Professor Bashforth, in his first letter to the Ordnance Select Committee, *recommends the adoption of a kind of printing telegraph . . . . in order that we may have a check upon the measurements of striking velocities.*" It goes on to state, page 25: "As this suggestion did not appear to meet with approval, it devolved upon Mr. Bashforth to carry out his own plans, which have now been brought to a successful termination after four years of incessant labour." (July 23, 1868.) In the same page the Report further declares: "The state of our knowledge of the resistance of the air in 1865 was well expressed in Captain W. H. Noble's Report to the Ordnance Select Committee dated April 2, 1865:—"It is regretted that this subject cannot be fully treated in the present Report, but the difficulties in the way of a clear solution are so many and so great, that it would be difficult with our present experience to assign any new law representing with *accuracy* the resistance of the air to the motion of spherical and elongated projectiles'" (page 19).

The scientific referees thus characterise the Bashforth instrument:—

"We do not think that any means before existed of recording a number of successive small intervals of time with the degree of precision and trustworthiness attained by Professor Bashforth's instrument."

* * * *

This instrument gives records in parallel spiral lines traced on a revolving cylinder:—(1) of every alternate beat of a half-seconds clock; (2) of the instants in 2,000ths of a second of the ball cutting threads in ten to fifteen screens placed 50—150 feet apart.

The accompanying diagram is a reduced *fac-simile* of the record of an observed round of firing cannon-shot through the screens.

Throughout the whole of these experiments, recorded permanently upon blue glazed paper, an elaborate system of testing minute working errors, by differences of a high order of scrutiny, approved by the first mathematicians of the age, has been put in practice. About 400 rounds have been tabulated: even corrections for the density of the air for observed readings, height of the barometer and thermometer, have been introduced.

The precision and excellence of this mode of testing by successive differences may be illustrated in the following manner.

If the velocity of the shot follows a particular law for certain limits, the successive differences will show its existence and any departure from it: just as a succession of squares and cubes tabulated with the slightest error can be thus detected:—

SQUARES. (ERROR.*)

| | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Nos. | 1 | | 4 | | 9 | | 16 | | 25 | | 36 | | 49 | | 64 | | **82** | | 100 |
| 1st differ. $\Delta_1$ | | 3 | | 5 | | 7 | | 9 | | 11 | | 13 | | 15 | | **18** | | **18** | |
| 2nd differ. $\Delta_2$ | | | 2 | | 2 | | 2 | | 2 | | 2 | | 2 | | **3** | | **0** | | |
| 3rd differ. $\Delta_3$ | | | | 0 | | 0 | | 0 | | 0 | | 0 | | 1 | | −3 | | | |

CUBES. (ERROR.†)

| | | | | | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Nos. | 1 | | 8 | | 27 | | 64 | | 125 | | 216 | | **342** | | 512 | | 729 | | 1000 |
| 1st differ. $\Delta_1$ | | 7 | | 19 | | 37 | | 61 | | 91 | | **126** | | **170** | | 217 | | 271 | |
| 2nd differ. $\Delta_2$ | | | 12 | | 18 | | 24 | | 30 | | **35** | | **44** | | **47** | | 54 | | |
| 3rd differ. $\Delta_3$ | | | | 6 | | 6 | | 6 | | **5** | | **9** | | **3** | | **7** | | | |
| 4th differ. $\Delta_4$ | | | | | 0 | | 0 | | **−1** | | **4** | | **−6** | | **+4** | | | | |

Now if the squares had been all multiplied by some constant coefficient, the last differences would have been equally reduced to zero. The same thing would have happened with the cubes. And, more complex still, if a new series were formed with terms consisting of squares and cubes, or their constant multiples, a series of differences would detect any deviation from a fixed law.

Further, the law, being ascertained by a great number of experiments, slight errors of observation could be detected and rectified by the principles of *interpolation*.‡

The Bashforth chronograph, actuated by a fly-wheel of

* An error of 1 is purposely introduced to show its exaggerated effect; the third differences, 0, 0, 0, 0, 0, 0, 0, being changed into 0, 0, 1, −3.

† An error of 1 only is introduced purposely, viz. 342 instead of 343, and great departure from regularity is shown in the third and fourth *differences*, in consequence of the above error.

‡ Even when a progressive law is unknown and implied, by which a series of results are obtained, still by successively differencing, by means of *equidistant arguments*, the existence of isolated errors can be speedily discovered and corrected.

nearly uniform velocity, and recording clock-beats as well as screen-striking within the 2,000th of a second (the projectile cutting the threads of ten or fifteen successive screens, placed 50—150 feet apart), gives a permanent comparative record of the instants of the passage of the ball. The following example is expanded from "Trans. of the Royal Society," 1868, p. 425.

Hemispherical-headed shot. Diameter 4·7 in.; weight 39·34 lbs.

| CLOCK | | | | |
|---|---|---|---|---|
| Time | Reading | *Uncorrected* Reading | | |
| | | $\Delta_1$ | $\Delta_2$ | $\Delta_3$ |
| 1″ | 4·910 | + 15·910 | | |
| 2″ | 20·820 | + 15·830 | − 80 | + 40 |
| 3″ | 36·650 | + 15·790 | − 40 | − 50 |
| 4″ | 52·440 | + 15·700 | − 90 | + 40 |
| 5″ | 68·140 | + 15·650 | − 50 | |
| 6″ | 83·790 | | | |

| CLOCK | | | | | |
|---|---|---|---|---|---|
| Time | Reading | *Corrected* Reading | | | |
| | | | $\Delta_1$ | $\Delta_2$ | $\Delta_3$ |
| 1″ | 4·91 | 4·910 | 15·910 | 70 | 3 |
| 2″ | 20·82 | 20·820 | 15·840 | 67 | 3 |
| 3″ | 36·65 | 36·660 | 15·773 | 64 | 3 |
| 4″ | 52·44 | 52·433 | 15·709 | 61 | |
| 5″ | 68·14 | 68·142 | 15·648 | | |
| 6″ | 83.79 | 83·790 | | | |

By interpolation * the instant of passing ten screens in succession, as marked by the spiral traced on the revolving drum, is thus given in decimals:—

TIMES OF PASSING THE SCREENS.

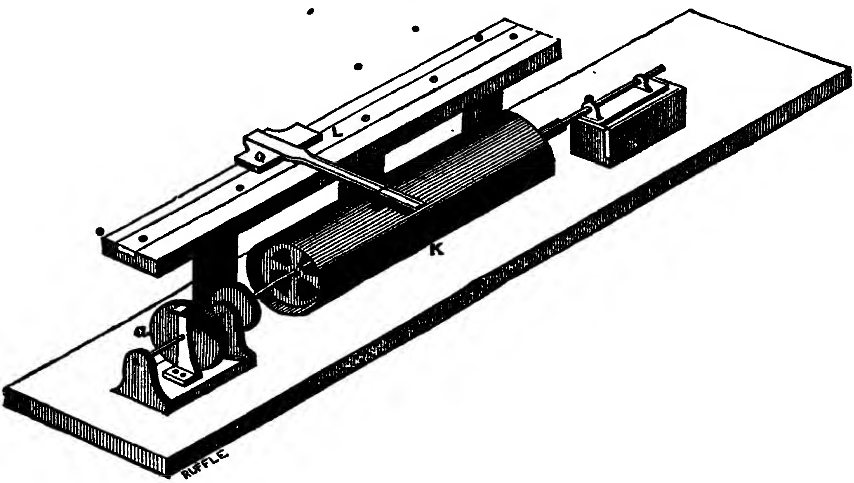
| Screen | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |
|---|---|---|---|---|---|---|---|---|---|---|
| Times | 2″·4692 | 2″·5956 | 2″·7238 | 2″·8539 | 2″·9858 | 3″·1196 | 3″·2552 | 3″·3925 | 3″·5315 | 3″·6722 |
| $\Delta_1$ | | 1264 | 1282 | 1301 | 1319 | 1338 | 1356 | 1373 | 1390 | 1407 |
| $\Delta_2$ | | | 18 | 19 | 18 | 19 | 18 | 17 | 17 | 17 |
| $\Delta_3$ | | | +1 | −1 | +1 | −1 | −1 | 0 | 0 | |

The length of the spiral traced on the paper cylinder is about nine or ten inches for one second; thus by a vernier and

* It is hardly desirable to insert the formulæ of this process in an article on Popular Science.

steel T-square and fine mark, the cylinder being removed from the machine and applied between centres as in the wood-cut, the actual times of flight are compared with the instants of time measured on the paper, correctly, to the 200th of an inch.* Next the velocities for the middle point of each interval between the screens are calculated. Data are then obtained for

Fig. 2.




comparing the changes in velocity with those which ought to arise from a supposed law of resistance for every ten feet of the projectile's flight. The agreement of the calculated with the experimental results is then verified.

From these researches, which, indeed, may be said to form a new era in gunnery, it appears as the result of chronographic experiments with some hundreds of rounds of every kind of shot used in the service—

That the diameter being the same, the shot preserves its velocity to a greater distance for hitting the mark, as the weight is greater.

That the resistance is less for the same weight as the shot is elongated within certain limits.

That the resistance also varies inversely as the square of the diameter.

That the resistance of the air for velocities used in practice (900–1700 f. s.) cannot be expressed by any simple power, or

* The steel T-square slides along the plate L and the mark *b* upon it being placed accurately upon the successive records of *seconds* and screens, is read off by the vernier (a) to about the $\frac{1}{200}$th of an inch, or 2,000ths of a second. The cylinder *k* is moved from the chronograph before the paper record is disturbed.

simple function, of the velocity. The resistance of the air may be taken to vary as the SIXTH power of the velocity from 950–1050 f. s., as the THIRD power from 1070 to 1400 f. s., and as the SECOND power for higher velocities. Under these circumstances the cubic law, with a varying coefficient, has been adopted as the most convenient for calculation.

That at 1,200 feet per second velocity this coefficient of resistance is the greatest for elongated projectiles, which rises in value rapidly at 1,000 f. s. to 1,100, and diminishes gradually as the velocity is increased beyond 1,200.

But for spherical shot the coefficient of resistance rises more gradually to its maximum at the same velocity of 1,200 f. s., and diminishes gradually for greater velocities.

DISSECTED TABLF.

| | | Chilled Shot | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Size and weight of shot | 15-in. Rodman | 150lb. | 100lb. | 68lb | 32lb. | 18lb. | 12lb. | 9lb. | 6lb. | 3lb. |
| Initial velocity | 2100 | 2100 | 2100 | 2100 | 2100 | 2100 | 2100 | 210: | 2100 | 2100 |
| Distance from gun | 8000 | 7000 | 6000 | 5000 | 4000 | 3600 | 3100 | 2700 | 2300 | 1900 |
| Corresponding velocity at that distance | 999 | 910 | 906 | 898 | 884 | 857 | 854 | 874 | 884 | 873 |

Such a table as this for every hundred feet is of high value, being the first ever trustily obtained.*

It is remarkable that no Artillery Manuals hitherto extant give any reliable information upon the decrease of velocity at different ranges. The old difficulty, the unknown irrepressible stumbling-block, the resistance of the air, vitiated every attempt at scientific conclusions.

* Its great value may be at once seen from the solution of innumerable questions like the following:—

A 6-pr. and a 32-pr. are both about to be fired with the same initial velocity of 2,100 f. s.: at what distance will the 32-pr. hit an object with the same velocity as the 6-pr. at 2,300 feet? By the table we see 4,000 feet distance has a velocity for the 32-pr. 884 f. s., and for the 6-pr. at 2,300 feet has a velocity of 884.

Again, a 15-inch Rodman keeps up its velocity at 8,000 feet equal to that of a 68-pr. at 4,100 feet, target distance.

Tables have also been ascertained for finding the velocity generated by different charges, so that the following complicated question can now be solved:—

A shot from a 9-inch gun (250 lbs.), fired at a target 200 yards' distance, is required to strike with the same velocity as it would strike at 1,000 yards, so as to compare perforating powers: what charges of powder must be used in each case?

When the celebrated gun of Sir William Armstrong, F.R.S., was first tested, the range was found to greatly exceed what it ought to have been by theory. Gunnery was all at sea for high velocities. The first elements, obtainable only by experiments at successive points of the path *of one and the same ball* were really unknown.

It is stated in these Reports (page 20):—

"Round 68, with 5-inch rifled bronze gun, permanently injured the gun." . . . "Nothing wrong was noticed before the eighty-fourth round. The resistance of the air was found to be *three* times its proper amount." *This diagnosed unsteadiness.* The shot was tilting itself in its flight, creating greater resistance. Again they say: "When we came to experiment with the new 5-inch M.L. gun, converted from a breech-loader, we found the resistance of the air for shot fired from this gun less in proportion than for any other calibre. We could only ascribe this reduced resistance to superior steadiness of the shot. The gun, when tried on long ranges, made remarkable good practice. Hence it is manifest that any chronograph which is capable of recording, in a satisfactory manner, the time at which the shot passes a succession of equidistant screens, affords a ready means of testing the degree of steadiness imparted to the shot by any new gun or new system of rifling."

The enormous labour of comparing each velocity at each of the screens for a considerable number of rounds may be easily imagined.*

The results of these beautiful experiments and researches are of the utmost importance—

(1.) In the technical education of artillery officers.

(2.) In the assistance they give to the practical artillerist.

The ascertained laws of flight will now present artillerists with a true guide for the performance of their arduous duties. The result of 18 rounds of shot through 10 screens, commenced

* If 120 feet be the distance of one screen from another, and the cubic law of resistance be applied, the actual time occupied by a 12 lbs. Armstrong shot passing over the interval $t = \cdot 000869520 \times 120 + \cdot 000000042 \times 120 \times 120$ Next, the velocity will equal *unity* divided by a quantity represented by $\cdot 000869520 + \cdot 000000042 \times 120$. In this case the resistance will vary as the cube of the velocity.

When 4,000 resistances at 10 screens for 400 rounds have thus been calculated, further corrections are required for the height of the barometer and temperature, and in each case the weight of the shot has to be an element of the calculation. The Report represents about five years of spontaneous labour, of a nature which few would like to undertake.

The system of successive differences to ascertain errors of observation requires the results to be supplemented with an elaborate process of interpolation, described in the Transactions of the Royal Society, 1868.

November 23, 1865, gave such indications of the power of the new method, that no subsequent correction or modification has been found necessary to adapt them to the law of resistance ascertained in several hundred rounds.

The eminent referees already named establish Professor Bashforth's claims to priority in the following sentence:—"By experiments made in this way, Captain W. H. Noble has obtained very good results, which confirm in a remarkable manner those which had been previously obtained by Professor Bashforth for high velocities, and agree approximately with those of M. Helié for lower velocities." A singularly interesting result has been obtained.

It appears that if the shot flies faster than a sound-wave, the shot is preceded by an automatic wave caused by itself; followed as it were more slowly by the wave of the explosion. But if the shot is behind the sound-wave, in consequence of its velocity being less than that of sound, the shot may be travelling in a medium more or less dense than the undisturbed atmosphere; and the law of resistance may be subject to sudden changes or leaps showing unsteadiness, detected by the readings of the chronograph. In this case tilting of the shot may be presumed.

There are several very valuable results obtained from the development of the ascertained laws of flight, and applicable to hitting the mark with the best effect.

Spheroidal-headed shot showed the greatest steadiness and the least resistance of all the solid forms of elongated shot used.

A condemned 40-pr. B. L. gun was bored out to 5 inches. The chronograph showed remarkable precision and steadiness of flight. "No other gun succeeded so well." Rounds 139—178 were fired April 29 and 30, 1868. The co-efficient of resistance was found remarkably low for this gun.

The 250 lbs. shot with $\frac{1}{12}$ charge of powder appeared to meet with unusually great resistance of the air, and indicated great unsteadiness for low velocities.

The 9-inch gun, formed with a rifling of 0 to 1 in 45, gave great indications of unsteadiness.

A 10-inch projectile would strike a harder blow at 2,000 yards than the 9-inch would inflict at the muzzle.

The power of perforation at 1,500 yards is the same as that of the 9-inch at the muzzle.

The Whitworth elongated 150 lbs. flat-headed shot appeared to exhibit unsteadiness of flight under the larger charge of 20 lbs. powder, but assumes an eminently true motion when fired the lowest charge, 10 lbs. of gunpowder.

The new Indian bronze gun, 3-inch 9-pr., also exhibited greater unsteadiness when fired with $1\frac{3}{4}$ lb. of powder than with $1\frac{1}{2}$ lb. charge. It is remarkable for both these guns, that the resistance of the air is most variable when the velocities are the highest. It has generally been found that accurate practice, or "beautiful hitting," has been best obtained when the charge is exactly suited to the weight of the ball, so as to fire it with the least irregularity of atmospheric resistance and greatest steadiness of flight.

This resistance, according to Professor Bashforth's researches, acquires enormous proportions (Report, p. 121).

TABLE (*abstracted*)

Showing the resistance of the air in lbs. to spherical shot from 1 to 15 inches in diameter, at velocities varying from 900 to 2100 f. s. (feet per second), calculated by means of Table V.

| Velocity f. s. | Resistance | | | | | | | Velocity f. s. |
|---|---|---|---|---|---|---|---|---|
| | 3 in. | 5 in. | 7 in. | 9 in. | 13 in. | 14 in. | 15 in. | |
| | lbs. | lbs. | lbs. | lbs. | lbs. | lbs. | lbs. | |
| 900 | 28 | 78 | 153 | 254 | 529 | 613 | 704 | 900 |
| 1300 | 91 | 252 | 494 | 817 | 1705 | 1977 | 2269 | 1300 |
| 1700 | 166 | 461 | 903 | 1493 | 3115 | 3613 | 4148 | 1700 |
| 2100 | 257 | 713 | 1398 | 2311 | 4823 | 5593 | 6421 | 2100 |

TABLE OF STRIKING VELOCITIES AT DIFFERENT DISTANCES ON HITTING THE MARK.

Diam. of shot, 8·92 inches. Weight of shot, 250 lbs.

| Distances from gun | Velocities, feet per second | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 0 | 100 | 200 | 300 | 400 | 500 | 600 | 700 | 800 | 900 |
| feet | f. s. | f. s. | f. s. | f. s. | f. s. | f. s. | f. s. | f. s. | f. s. | f. s. |
| 0 | 1400 | 1394 | 1387 | 1381 | 1375 | 1368 | 1362 | 1356 | 1350 | 1344 |
| 1000 | 1338 | 1332 | 1326 | 1320 | 1315 | 1309 | 1303 | 1298 | 1292 | 1287 |
| 2000 | 1281 | 1276 | 1271 | 1265 | 1260 | 1255 | 1250 | 1244 | 1239 | 1234 |
| 3000 | 1229 | 1224 | 1219 | 1214 | 1209 | 1205 | 1200 | 1195 | 1191 | 1186 |
| 4000 | 1181 | 1177 | 1172 | 1167 | 1163 | 1159 | 1154 | 1150 | 1145 | 1141 |
| 5000 | 1137 | 1133 | 1128 | 1124 | 1120 | 1116 | 1112 | 1108 | 1104 | 1100 |
| 6000 | 1096 | 1092 | 1088 | 1085 | 1081 | 1077 | 1073 | 1070 | 1066 | 1062 |
| 7000 | 1059 | 1055 | 1052 | 1048 | 1045 | 1041 | 1038 | 1035 | 1031 | 1029 |
| 8000 | 1025 | 1021 | 1018 | 1015 | 1012 | 1009 | 1006 | 1003 | 1000 | 997 |
| 9000 | 994 | 991 | 988 | 985 | 983 | 980 | 977 | 974 | 972 | 969 |
| 10000 | 967 | 965 | 962 | 960 | 957 | 955 | 953 | 950 | 948 | 945 |

TABLE FOR REGULATING STRIKING VELOCITY BY ALTERING THE WEIGHT OF POWDER CHARGE.

| Charge | Initial velocity | | Charge | Initial velocity | | Charge | Initial velocity | |
|---|---|---|---|---|---|---|---|---|
| lbs. | f. s. | $\Delta_1$ | lbs. | f. s. | $\Delta_1$ | lbs. | f. s. | $\Delta_1$ |
| 20 | 959 | + 31 | 29 | 1171 | + 16 | 38 | 1274 | + 8 |
| 21 | 990 | + 29 | 30 | 1187 | + 15 | 39 | 1282 | + 8 |
| 22 | 1019 | + 27 | 31 | 1202 | + 13 | 40 | 1290 | + 8 |
| 23 | 1046 | + 25 | 32 | 1215 | + 12 | 41 | 1298 | + 8 |
| 24 | 1071 | + 24 | 33 | 1227 | + 11 | 42 | 1306 | + 8 |
| 25 | 1095 | + 22 | 34 | 1238 | + 10 | 43 | 1314 | + 8 |
| 26 | 1117 | + 20 | 35 | 1248 | + 9 | 44 | 1322 | + 7 |
| 27 | 1137 | + 18 | 36 | 1257 | + 9 | 45 | 1329 | |
| 28 | 1155 | + 16 | 37 | 1266 | + 8 | | | |

The following paragraph from the *Pall Mall Gazette* will not form an uninteresting conclusion to my observations: "In the recent great sortie made by the French from Paris, General Ducrot brought into action one of those new engines of destruction to the invention of which the present war has given so great an impetus. This is an armour-plated locomotive, furnished with two powerful mitrailleurs, also protected by armour, and originally intended for the railway bridge at Point du Jour, whence it was to throw bullets on to the heights of Meudon. This novel machine, which weighs altogether only some six tons, has been manufactured at Cail's, the well-known mechanical engineer of Paris, to whose establishment the city is so much indebted for the extraordinary efforts that have been made to supply it with cannon and other means of defence. The Prussian invasion has certainly contributed a great deal to develope the inventive talents of the French; for hardly a day passes without some new implement of destruction being submitted to the Government of National Defence. Under the spur of defeat they have produced the Marekderberg mitrailleur, firing 250 balls a minute, and the Montigny, firing 480; as well as the Durant steam mitrailleur, which discharges no less than 4,500 in the same space of time, and the Faucheuse, or 'mower,' which is said to operate without noise, smoke, or fire, to have a range of from 500 to 600 yards, and to cost only 35*f.*, with all the necessary apparatus for firing 300,000 projectiles; so that, if every bullet really had its billet, the French, by employing this weapon, might rid themselves of the whole of their enemies for something less than 100*f.* In addition to the above, many novel descriptions of shells have also been proposed, if not actually tried, among which are the Gaudin fire-bomb, the improved Menestrol shell, bombs emitting suffocating vapours, and so on."

This state of things renders long range accurately striking artillery of the utmost consequence at the present time.

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## DESCRIPTION OF PLATE LXVII.

The Chronograph is actuated by a fly-wheel A.

K is a cylinder covered with prepared paper for receiving the record made electrically by the beat of the clock, and the cutting the threads of the several screens through which the shot passes in its flight. The cylinder is about 12 inches long; diameter 4″.

B a toothed wheel, gearing with M so as to allow a string CD to be slowly unwrapped from its drum, whilst the other end is attached to the sliding platform L, moving about a quarter of an inch for each revolution.

EE′ electro-magnets. *dd′* are frames supporting the keepers, *f*, *f*,′ the springs acting against the magnetic attraction.

On interrupting the current, by the destruction of magnetism the spring *f* carries back the keeper, which causes the arm *a* to strike a blow on the lever *b*. Thus the marker *m* is made to depart from the uniform spiral it was describing. But when the current is restored, the keeper being attracted causes the marker *m* to be brought back, and so continue to trace the spiral as if nothing had occurred to alter its course.

E′ is connected with the clock, and its marker *m′* records seconds.

E is connected with the screens, and records the passage of the shot through each of them in succession.

Thus, by comparing the marks made by *m m′*, the exact velocity of the shot can be calculated at all points of its course. The slide L is fixed parallel to F and the cylinder K by the brackets G, H.

J is a stop to regulate the distance between wheels M and B, and Y draws back the wheelwork M. The depression of the lever *h* raises the two springs *s*, which, acting as levers, bring the diamond points of the markers *mm′* down upon the paper.

When an experiment is to be made, the fly-wheel A is set in motion by hand, so as to revolve about three times in two seconds; the currents are completed, the markers *mm′* are brought down to the paper cylinder K and after four or five beats of the clock the "signal to fire" is given, so that in about ten seconds the experiment is completed and the instrument is ready for another.

The passage of the balls through the fifteen screens is distinctly recognised by the ear, a peculiar tr-r-r-r-rap being heard before the sound of the explosion, if several hundred yards away.

# NATURAL SELECTION INSUFFICIENT TO THE DEVELOPMENT OF MAN.

By the Rev. GEORGE BUCKLE, M.A.

IN a well-known passage towards the close of the "Origin of Species," Mr. Darwin supposes the question to be put to him, How far does your doctrine extend, and what amount of ground does it cover? The answer is perfectly frank and clear. Practically it covers the whole area of life. Every class, at least of animals and plants, must own a common ancestor, and probably these class-founders are themselves only brethren descended from some yet remoter stock. Of the former of these two positions he speaks confidently. "I cannot doubt," he says, "that the theory of descent, with modification, embraces all the members of the same class. I believe that animals have descended from at most only four or five progenitors, and plants from an equal or lesser number." Of the latter he speaks with more reserve. "Analogy would lead me one step further, namely, to the belief that all animals and plants have descended from some one prototype. But analogy," he adds, "may be a deceitful guide. Nevertheless he sees sufficient reason to justify him in following its guidance in this instance, and finally sums up his opinion in the following remarkable words: "Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed."

The natural inference from these words would be that Mr. Darwin considered his theory of natural selection as sufficient to account for all the varieties of life on the face of the earth. But it is not a necessary inference. For he is speaking, in this passage, not precisely of the doctrine of natural selection, but of the doctrine of "descent with modification;" and the two ideas are perfectly distinct. For it is quite possible that all living beings may be descended from a single primordial form, and yet that natural selection may not be the only

agency employed in the determination of their actual variety. Other methods and other forces may have conspired with it, checked or thwarted it, in the work of educing from one common form the boundless multiformity which now meets our eyes. No doubt the whole course of Mr. Darwin's reasonings and illustrations leads us to the conviction that in his judgment the unassisted action of natural selection is sufficient to produce all the necessary modifications, but so far as express words go, he has not excluded—at any rate in the passage which I have quoted—the possibility of the co-operation or interference of some other cause; and it is important to call attention to this, because a very high authority on this subject, Mr. A. R. Wallace—the independent originator, and the most able defender of the theory which bears Mr. Darwin's name—has recently proclaimed his conviction that natural selection by itself is inadequate to the production of at least one, and that the most important, form of life. In other words it is impossible, in Mr. Wallace's opinion, that man can have been developed from the inferior animals by the process of natural selection alone. Whatever else it may have done, it is unequal to this, the great and crowning act of creative power.*

To understand his reasonings we must first get a clear idea of what the doctrine of natural selection is. It does not imply, as many will persist in assuming, any capacity in the individual to alter his own structure, and adapt himself to surrounding circumstances. The individual does not materially change. Such as he is born, such, in his physical structure, he will remain to the end of his life. Only if his physical structure does not happen to be well adapted to the circumstances in which he finds himself, his life will be a short one. His neighbour, who happens, by some small variation, to be slightly better adapted to those circumstances, will live longer. And, moreover, since the offspring inherit the parents' peculiarities, the descendants of this latter are likely to prevail to the exclusion of those of the former; and thus, in the course of some generations, the prevailing type and character of the whole family will be slightly modified. It is not the individual, but the collection of similar individuals, or the Kind—a word which may be usefully employed to avoid the technical meaning attaching to class or species—that changes. And it changes only by means of changing its units, by dropping out from time to time those that are unsuitable, and keeping in and preserving those that are suitable. In this way it adapts itself

* See "Contributions to the Theory of Natural Selection." (Macmillan & Co.) By A. R. Wallace. This paper is little more than an expansion of part of the argument in one of these Essays.

to the perpetual changes of surrounding circumstances, and keeps itself by its own variations in constant harmony with the ever-varying world around it. As earth and seas, forest, river and meadow, climate and temperature, are never for a moment stationary, but maintain a perpetual ebb and flow of ceaseless interchange, so the general forms and types of life, which are affected by all these influences, are also in continual and corresponding flux. Both are always in a condition of instability in themselves, because both are always in perfect harmony with each other. But it follows from this that no modification can possibly be introduced into any form or type of life, unless it be beneficial to the creature modified—unless it tend, in some way or other, to bring him more into harmony with the conditions around him than he was before. If the change be merely a matter of indifference, doing neither good nor harm to the possessor, it will make no impression on the Kind. It is an individual peculiarity which may re-appear again here and there in other individuals, but which has no tendency to prevail over other similar peculiarities in others. But if the change is actually injurious, it will vanish at once. The unlucky possessor of it will be inferior to his neighbours in the struggle for existence; his life will be cut short sooner than that of others; his offspring, if they inherit his peculiarity, will inherit also his disadvantages, and will soon perish out of the Kind, leaving no trace behind them. Natural selection is like fortune; it favours only the brave; it helps those only who can help themselves; it rejects the weak, the puny, the ill-provided, and ill-adapted; and its effect is best described as the survival of the fittest.

Now let us apply these principles to the case of man. Were the changes by which the Kind passed—if it did pass—from some lower type to the human type such as would be manifestly beneficial in the first instance to the individuals who were affected by these changes? Because, if they were not, that transition could never have been effected by natural selection. If it occurred at all, some other agency must be taken into account. What, then, were these changes? We cannot, of course, tell exactly, unless we knew—as we certainly do not know—the form of life which immediately preceded the human. But let us assume for the moment that the anthropoid apes and man are the extremities of divergent lines from some remote ancestor, uniting in himself the characteristics which they have in common; how would the differentiation begin to be carried on? One of the most marked peculiarities in man is the soft, smooth skin. Alone among the mammalia, he is unprotected either by the hardness or the shagginess of his integument. He has neither the impenetrable armour of the rhinoceros, nor the

thick fur of the bear, nor the warm wool of the sheep. Was it any advantage to the first individual that came into the world with this soft, smooth skin, or with any approximation to it, beyond his fellows? Was it a peculiarity likely to help him in the struggle for life—to enable him to survive when others perished?—likely, therefore, when transmitted to his offspring, to appear in greater force in the next generation; and gradually, by its superior adaptation to surrounding circumstances, to supplant the tough or hairy skins which had preceded and accompanied it? Was it likely, in short, to become an object of natural selection? Is it not, on the contrary, quite plain that the very reverse would be the case? The accidental possessor of this smooth skin would clearly be at a great disadvantage. He would succumb beneath the attacks of enemies which his hardier fellows could successfully resist. Rain and frost and cold would work their bitter will upon him unchecked. Inclement seasons, which only produced a moderate inconvenience, or none at all, to creatures with thick or shaggy hides, would soon prove fatal to the animal we are imagining. There is no conceivable reason why such an animal should live and perpetuate his peculiarity, while others which did not possess it perished; there is, on the contrary, every reason to suppose that such an animal, born for the first time, an anomaly in a shaggy world, would speedily be eliminated and leave no trace behind him. That is to say, it is impossible to picture a condition of things in which a kind of creatures distinguished by smooth skins could have arisen by the process of natural selection. In other words, natural selection cannot account for the origin of this peculiarity in the human form.

But that is not all. The theory of natural selection not only requires that every change promoted by it should be for the benefit of the possessor; it requires also that it should be for his *direct* and *immediate* benefit; that it should be no greater than is necessary to give him some instant advantage, however slight, over his fellows. For it does not act, any more than Nature herself, *per saltum*. It rests for its motive force upon the variation which always exists between a parent and an offspring; and this variation is, for the most part, very slight. It is enough to distinguish one from the other, but never much more. It is generally so small that the unpractised eye often fails to see any difference whatever. We do not mistake our friends for their fathers, though, if we do not know them well, we are liable sometimes to get confused between brothers and sisters; but, except to the shepherd, a flock of sheep seem to be all exactly alike. The differences between individuals of the same kind are for the most part very small, and it is only

on these differences that natural selection acts. Hence it happens that the transformation of one kind into another is a very slow and gradual process, because it has to be accomplished by a series of very small steps. A long step cannot be taken unless it is more to the advantage of the individual than a short step in the same direction, because it is certain that many more individuals will be born in any given generation with the small than with the large variation; and, unless the large one has some direct advantage over the small, the mere superiority of numbers will give the victory to the latter. Let us illustrate this by an example. Suppose a flower, such as the *Angræcum Sesquipedale* of Madagascar, with a very deep nectary, and a supply of nectar at the bottom of it. This can only be reached by a moth with a very long proboscis. Suppose also that this nectary has, from any cause, a continual tendency to lengthen in successive generations. It is evident that moths that happen to be born with probosces longer than the average will have an advantage over those that are born with them shorter. They will have at least, other things being equal, one more flower to feed on, and so have a better chance for life. Natural selection will therefore operate to produce a Kind of moths with long probosces. But it will not give any preference to a proboscis longer than is required for that special purpose. A proboscis which has an inch to spare would not be a bit more useful than one which could just drain the nectar and no more. And while many moths would be born with the slight additional length necessary for this, few or none would be born with the proboscis an inch longer. Such moths would be monstrosities, and monstrosities are always rare. And there would be no cause at all tending to perpetuate such a monstrosity and to counteract the universal tendency in all such cases to return, if unchecked, to the normal type—a tendency which is, in point of fact, simply another expression of the perpetual effort, which all life manifests, to bring itself into absolute harmony with all around it. The music of the spheres will not tolerate a discord; if a half-note too high or too low can be caught occasionally by the listening ear, it is soon swept out and lost in the full strong current of advancing sound. The office of natural selection is to maintain this concord, and it does it by favouring those slight variations which, by bringing their possessor more into harmony with the world around, give him an instant advantage over his fellows. It does not favour any larger variations; it has no forecasting eye to the possibilities of any future advantage to be derived from them.

Now let us apply this principle once more to the case of man, and in so doing let us pass from an external and super-

ficial to an internal and very forcible characteristic. The smooth skin is an obvious and striking peculiarity of man; but if anyone were asked what above all else made him what he is, he would probably reply, the brain. Let us see, then, if it seems likely that the human brain was developed by natural selection from the brute brain. The size of the human brain is, in comparison with that of all other animals, enormous. This superiority in magnitude, accompanied as it is by certain other less obvious and less indisputable marks of difference, seemed to Professor Owen sufficient to justify him in placing man in a class by himself—that of Archencephala, or chief-brained animals. The average brain of the highest anthropoid apes—the orang-utan or the gorilla—does not reach above 28 or 30 cubic inches, while the average internal capacity of the cranium in the Teutonic family of man amounts to 94 cubic inches. The difference is enormous; but if we could trace the growth of that difference step by step from one to the other, and see how at every step the owner of the larger brain would gain thereby an advantage over the smaller, there would be nothing in this difference to take it out of the ordinary action of natural selection. If the primitive flint-chippers had brains not much larger than apes, if those of the modern savages were a little bigger still, and if, as we travelled towards the civilised and intellectual periods of history, we found the brain steadily increasing, the change would be in full accordance with other illustrations of the law. But what is the case? So far as investigation has yet gone, there is no great difference in the average cranial capacity of man under any circumstances. That of the Esquimaux is 91 cubic inches, of the Negro 85, of the Australians and Tasmanians 82, while even that of the Bushman—the lowest specimen of living humanity with which we are acquainted—is 77. Nor do the few skulls of the earlier races, which have yet been discovered, tell any different tale. The celebrated Engis skull, which was probably contemporary with the mammoth and the cave bear, has been pronounced by Professor Huxley to be "a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." But the brains of any ape would have lain in a corner of it, and left a large vacancy. If the ape passed into the savage, the change in the brain was made by a leap. Now is there anything to make such a leap likely? Is there anything in this enormous increase of brain which would give its possessor an advantage over smaller brains, and enable him to survive while they perished? No doubt a larger brain has an advantage over a smaller one. The brain is the organ of the greatest power that we know—the power of mind. It is the

seat of thought, intelligence, sensation, emotion, will. He who owns these mighty implements in larger measure than his fellows has no doubt a great advantage over them in the struggle for existence, *if he uses them.* But they are no good to him in this respect while they lie latent or unused. A man does not become a match for a wild beast because he has a spear laid up in his armoury at home. The spear must be in his hand, and driven by strong muscles into the heart of his foe, to be of any use to him.. So it is with the mental faculties. Just so much as a man *uses* of them would become the object of natural selection, and no more. All the surplusage goes for nothing in the battle of life. The largest gorilla brain that has yet been measured contains 34½ cubic inches. Probably mental power depends on some other conditions besides the mere size of the brain, and therefore we should not be justified in saying that a creature with 35 inches of brain would certainly beat this gorilla. But we know that size is a principal factor in the problem, and we may therefore say very confidently that 40 inches of brain would answer this purpose. How, then, does it happen that the lowest savage has more than 70? Natural selection might secure him the 40, because apes with less brain would be crushed out to make room for him; but how would he get or keep the additional 30? If an individual chanced to be born, a mere monstrosity, with this huge addition to the normal quantity of his kind, what likelihood would there be of its being perpetuated? He would be simply in the condition of the moth with its proboscis an inch longer than was required for any useful purpose, and the sure result—if natural selection were the only power that acted upon it—would be the rapid reversion of his descendants to the ordinary type.

But, it may be asked, is all this brain so much surplusage in the savage? Are we justified in assuming that the greater portion of it lies dormant? Are we sure that he does not use it all, and that, in this use of it, there does not lie the secret of his superiority over the brutes around him, and the germ of that dominion over the whole creation which seems to be the goal to which he is continually tending? The only answer to this can be found in the comparison of the savage as regards the action of mind, on the one hand, with the highest of the brutes beneath, and, on the other, with the civilised man above him. If the difference in the amount of brain corresponds in these three gradations with the difference in mental development, the inference would be that the whole brain was used in each case. If this correspondence does not exist, it will follow that the brain is unused in any case in the degree in which the mental development in that case falls short of its required

proportion. Now the average proportions of the brain in the anthropoid apes, in the savage, and in civilised man respectively, may be represented by the figures 10, 26, and 32. Is this a true representation of the mental conditions of the three? Is the difference between the savage and the brute really more than twice as great as that between the savage and the educated European? Mr. Wallace bids us think of the difference in mathematical power between a senior wrangler and an average Englishman, and then descend from that to the condition of a savage who cannot count beyond three or five—of the mental wealth and vigour implied in forming abstract ideas, carrying on chains of complicated reasoning, and transacting the manifold business of law, commerce, and politics in our modern life on the one hand, and of the meagreness and poverty of savage life on the other, wholly given up to the mere necessities of providing daily food—and then say whether the intellectual development of the savage is not much more nearly akin to that of the lower animals around him than to that of the cultivated European. But if so, a large part of his enormous development of brain is simply wasted. He gets no good from it, and therefore there is no reason, on the principle of natural selection, why it should have grown so large. For natural selection can only favour the increase of any particular organ just so far as that increase confers an actual benefit in the struggle for existence. If the increase of the organ outgrows its use, that additional growth is due to some other cause; for natural selection admits no surplusage.

Nor is the size of the brain the only characteristic in man which presents this difficulty. Mr. Wallace applies the same line of argument with great ingenuity to the foot, the hand, the voice, and, above all, the higher mental faculties. All these seem to be perfected and specialised far beyond their actual needs in savage man. The upright gait of man, "god-like erect," the delicate capacities of his hand, the vocal apparatus capable from the first of the exquisite modulations which can only be appreciated by the cultivated ear, the moral sense, the perception of beauty, the abstract conceptions of number and extension—all these seem wholly out of the range of the results that can be accounted for by the preservation of useful variations. They all point in a very different direction, and lead us on to another stage in Mr. Wallace's argument.

For it is remarkable that all those peculiarities, which seem, like the large brain, to be superfluous, or, like the smooth skin, to be positively injurious, to their first possessor, are eminently qualified to lead man on to the heights of being which he has subsequently attained. The smooth skin suggests at once the necessity of clothes; the absence of claws and

talons, combined with the wonderful capacity of the hand, leads naturally to the fabrication of tools and weapons; the vast size of the brain provides a dormant reservoir of intellectual power, out of which every need, as it arises, may be met by a corresponding contrivance of supply. But all these capacities have a reference to the future, and not to the present. In the first instance, we see a creature born into the world weak, undefended, and unsupplied for the moment, but provided with faculties which eminently fit it for a far higher existence in some remote ages and under very different conditions. The capacities are given first; the use of them comes later. They do not arise out of the pressure of past necessity; they are bestowed in anticipation of future wants and for the furtherance of a future development. But that is the method of final causes, which is exactly contradictory to that of natural selection. The former looks always forwards, and the latter looks always backwards. The one is the method of prophecy, and the other of history. The one implies the action of an intelligent and forecasting agent, while the other relies wholly on a chain of causation—which may or may not have been established in the first instance by an intelligent agent, but which, once established, works on blindly and unalterably by itself. This may be illustrated by the action of man upon Nature in his own province of artificial selection. When the florist wishes to produce a particular variety of flower or leaf, he carefully selects all individuals that approximate towards it, guards them from injurious influences, secures their inter-breeding, and takes them, in short, by his protecting care out of the natural conditions into which they are born. The pigeon-fancier aiming at a special feather, the poultry-breeder desiring to secure plenty of eggs, the sheep-farmer cultivating specially, as it may happen, wool or mutton, acts in the same way. In all these cases an ideal is first proposed which is afterwards worked up to. The ordinary operations of Nature are defied or counteracted by special contrivance in order that the proposed end may be gained—that the intended type of animal may be, so to speak, created. They are all cases, within narrow limits, of final causes, in which man's intelligence is the causer, and the laws of Nature the unintelligent instruments. Natural selection has, in these cases, to bow before the higher power of human selection. The inference which Mr. Wallace draws from the line of thought which he has developed—and it seems the only possible inference—is that some such superior selection has been at work in the production of man. Some higher intelligence has exercised over the world at large the same kind of control which man displays in his farm or in his poultry-yard. This superior intelligence has forced the great life-agencies on the

earth out of their natural course for the sake of producing a choice and eminent creature, just as the florist manipulates his roses to produce a Lamarque or a Maréchal Niel, or a pigeon-fancier his birds to bring about a pouter or a fan-tail. Into the further question of what this mighty Life-fashioner may be, or by what other name he may be called, Mr. Wallace does not enter, though we may gather, from a passage in which he speaks of "the controlling action of such higher intelligences," that he does not necessarily identify him with the First Cause of all things, but rather inclines to the view that such interference with the ordinary course of nature may be due to some unknown order of intelligent existences, the existence of which may help to carry our thoughts across the immeasurable chasm which separates man from the Infinite and Unconditioned.

These are thoughts which open vistas of scientific imagination in which even Professor Tyndall might find ample room to range. If we admit them at all, it is scarcely possible to stand still on them. If this overruling and intelligent selection has been necessary to produce man, why should it be limited to that single achievement? A unique and solitary interference of this kind is far more inconsistent with any philosophical view of creation than an habitual and regular guidance. Mr. Wallace himself puts this forcibly when he admits that his theory "has the disadvantage of requiring the intervention of some distinct individual intelligence, to aid in the production of what we can hardly avoid considering as the ultimate aim and outcome of all organized existence—intellectual, ever-advancing, spiritual man." But the disadvantage vanishes if he will boldly extend his theory, and allow it to include, as he hints in the following sentence, the idea "that the controlling action of such higher intelligences is a necessary part of the great laws which govern the material universe;" or, to put it in other words, that intelligent superintendence is a perpetual factor in the development of life. Other cases, besides man, might easily be brought forward, which present similar difficulties in the way of natural selection, and seem therefore to require the introduction of this other factor. What, for instance, were the steps which led to the production of the first mammal, or of the first vertebrate? It is easy to see the superiority of the perfect animal in either case, and its consequent fitness as an aim towards which intelligence might work, but very difficult to comprehend how the first steps in either direction can have been beneficial to the individual. Some years ago a Scotch clergyman, Mr. Rorison, published a little book, which has hardly been so widely read as it deserved to be, entitled "The Three Barriers." They were the Brain, the Breast, and the Backbone—the symbols of Wisdom,

Love, and Power—which he maintained to constitute insuperable barriers in the development of species by natural selection alone. Mr. Wallace has admitted the difficulty in the case of the brain; is he prepared to deny it in the case of the other two? He maintains—so far as appears at present, unanswerably—that man cannot have been produced by the unaided power of natural selection: does not that raise a strong presumption in favour of the introduction of another agent in other cases also? He has marked out very clearly and conclusively the limits of natural selection in the origination of species; can he set any limits to the controlling and interfering Power which he has invoked to fill up the deficiency?

# POLYMORPHIC FUNGI.

By M. C. COOKE, M.A.

[PLATE LXVIII.]

IT is now generally admitted that a great many fungi, formerly regarded as good and distinct species, are, in reality, only conditions or stages of other forms. It has been proved beyond doubt that many species of fungi are truly polymorphic, appearing under different phases. It is, notwithstanding all this, most premature and unjustifiable to conclude, as some have done, that there are no good species at all, or that there is no certainty whatever in the study. Whilst admitting that many of our old notions have been overturned, that what at one time we hardly deemed possible has been proved to take place, we are not prepared to go the length of some, whose knowledge of the subject falls far short of their assumption. It is not very long since that one writer gravely asserted his opinion that all the British species of *Æcidium*, for instance, would be reduced to a single species; that, in fact, there was no sound specific distinction between them. This opinion originated probably rather in prejudice than as the result of study and investigation. Others have lumped together a host of unassociated species, without satisfactory evidence, and declared them to be only the same thing under different conditions. Hasty generalisations in this, as in other cases, produce more harm than good.

It is exceedingly difficult to trace such minute organisms as fungi, especially moulds, and to prove, without doubt, that they are conditions, the one of the other. It is easy enough to sow the spores of a certain Mucedine on paste, or potato, or any other matrix, cover them carefully, and watch the result; then, if the common *Aspergillus* or *Penicillium* makes its appearance, to some minds it is at once conclusive that the said Mucedine is only a condition of *Aspergillus* or *Penicillium*. Such a conclusion is not only rash, but mischievous, and far from the truth. There is no evidence that the *Asper-*

*gillus* or *Penicillium* originated from the spores of the Mucedine which were sown, but perhaps never germinated. When two moulds proceed apparently from the selfsame mycelium, judgment may be pronounced too hastily, for the mycelium of both *may* be distinct, though interlaced together; the safest conclusion being based on two forms of fruit when developed upon the same thread. Beyond this, there is always room for doubt. Hence it will be seen how difficult it is to prove dimorphism in moulds under such conditions. In many cases it is more presumption than proof. These remarks are not made with the view of discrediting the conclusions of such observers as Professor De Bary and the brothers Tulasne, but rather as a caution against assuming as fact that which is only conjecture.

Messrs. Tulasne, in their splendid work, "Selecta Fungorum Carpologia," have given a great number of instances of polymorphism. We have no reason to doubt that in many cases, perhaps most, they are quite correct, but even some of their conclusions require verification before they can be accepted as established fact. As an illustration of the results determined with regard to one species by these authors, we may instance the very common *Sphæria* (*Plæospora*) *herbarum.* It occurs on the dead stems of herbaceous plants, on the leaves of some trees, and even sometimes on decaying *Algæ.* On pea and bean stems it is usually plentiful. In fact, it is almost the commonest *Sphæria*, and easily recognised. The sporidia are, of course, contained in elongated, transparent, membranaceous asci; they are of a yellowish-brown or amber colour, ovate-oblong, and divided by numerous septa, with transverse divisions. The asci are enclosed within carbonaceous perithecia.

Equally as common, and even more so, is a mould which forms sooty or dark olive spots, or patches, on all kinds of decaying vegetable substances. This is called *Cladosporium herbarum.* It may be characterised as cosmopolitan, and one of the commonest, if not the commonest, of fungi. Under the microscope this mould consists of a profuse mycelium, from which arise tufts of jointed threads, mixed with elliptical or elongated spores, ultimately septate. This mould is one condition, according to M. Tulasne, of *Sphæria herbarum.*

Another condition of the same plant is a very pretty mould found mixed with, or parasitic upon, the *Cladosporium*, and known as *Alternaria tenuis.* This species is figured in Corda's "Prachtflora," and consists of chains of spores resembling inverted jointed clubs. The joints are also transversely divided, as in the *Sphæria* sporidia.

A third form of the same species is that named by Rev. M. J. Berkeley *Macrosporium sarcinula*, which is developed

on decaying gourds. The spores are clavate, at length somewhat rectangular, with numerous septa, constricted, and very variable, both in size and in the number of cells.

Besides these, there are certain "distinct papillate, or bottle-shaped cysts, which contain naked spores, capable of germination." So that altogether we have five different forms of fungi, all of which are but stages or conditions of one and the same thing. It is very probable that, in addition to these, *spermatia* may also hereafter be discovered, or traced to some already known Coniomycetous species. From this example it will be readily understood what we mean when writing of "polymorphic fungi."

Having thus, as it were, defined our terms, we will proceed to notice two instances of apparent polymorphism which have come before us. We say "apparent" advisedly, because in the second instance only suspicions can be predicated. Some two or three years ago we collected a quantity of dead box-leaves, on which grew a mould named by Link *Penicillium roseum*. This mould has a roseate tint, and occurs in patches on the leaves; the threads are erect and branched above, bearing oblong, somewhat spindle-shaped, spores. When collected these leaves were examined, and nothing was observed or noted upon them except the *Penicillium*. After some time, certainly between two and three years, during which the box remained undisturbed, circumstances led to the examination again of one or two of the leaves, and afterwards of the greater number of them, and the patches of *Penicillium* were found to be intermixed with another mould of a higher development and far different character (Pl. LXVIII. fig. 5). This mould, or rather *Mucor*, for it belongs to the *Mucorini*, consists of erect branching threads, many of the branches terminating in a delicate, globose, glassy head, or sporangium, containing numerous very minute subglobose sporidia. This species has been named *Mucor hyalinus*. The habit is very much like that of the *Penicillium*, but without any roseate tint. It is almost certain that the *Mucor* could not have been present when the *Penicillium* was examined, and the leaves on which it had grown were enclosed in the tin box, but that the *Mucor* afterwards appeared on the same leaves, sometimes from the same patches, and from the same mycelium. The great difference in structure of the two species lies in the fructification. In *Penicillium*, of which the figure 4 of our plate (Pl. LXVIII.) is a good illustration, the spores are naked, and in moniliform threads, whilst in *Mucor* the spores are enclosed within globose membranous heads or sporangia, as shown in fig. 5. The moulds, or *Mucedines*, to which *Penicillium* belongs, are included in one of the large family of fungi termed *Hypho-*

*mycetes*, and the *Mucors* belong to another family, the *Physomycetes*. We entertain no doubt whatever that the *Mucor*, to which we have alluded as growing on box-leaves, intermixed with *Penicillium roseum*, is no other than the higher and more complete form of that species, and that the *Penicillium* is only its conidiiferous state. The presumption in this case is strong, and not so open to doubt as it would be did not analogy render it so extremely probable that such is the case, apart from the fact of both forms springing from the same mass of mycelium. In such minute and delicate structures it is very difficult to manipulate the specimens so as to arrive at positive evidence. If a filament of mycelium could be isolated successfully, and a fertile thread, bearing the fruit of both forms, could be traced from the same individual mycelium thread, the evidence would be conclusive. In default of such conclusive evidence, we are compelled to rest with the assumption until further researches enable us to record the assumption as fact.

In Lewis's recent "Report on Microscopic Objects found in Cholera Evacuations" (Calcutta, 1870), a similar instance of presumed dimorphism between precisely the same genera is thus recorded. "On a preparation preserved in a moist chamber on the third day a white speck was seen in the surface consisting of innumerable 'yeast' cells with some filaments, branching in all directions. On the fourth day tufts of *Penicillium* had developed—two varieties, *P. glaucum* and *P. viride*. This continued until the ninth day, when a few of the filaments springing up in the midst of the *Penicillium* were tipped with a dew-drop like dilatation, excessively delicate—a mere distended pellicle. In some cases they seemed to be derived from the same filament as others bearing the ordinary branching spores of *Penicillium*, but of this I could not be positive. This kind of fructification increased rapidly, and on the fourteenth day *spores* had undoubtedly developed within the pellicle, just as had been observed in a previous cultivation, precisely similar revolving movements being also manifested." Here we have another example of a *Mucor* developed from a *Penicillium*, and one observation strengthens and confirms the other.

Before entering upon the details of the second apparent polymorphism, it may be as well to give some particulars of the circumstances under which the fungi appeared. It was our fortune—*good* fortune as far as this investigation is concerned—to have a portion of wall in our dwelling persistently damp for some months; it was close to a cistern that became leaky. The wall was papered with "marbled" paper, and varnished. At first there was for some time—perhaps months—nothing worthy of observation except a damp wall; decidedly

damp, discoloured, but not by any means mouldy. At length, and rather suddenly, patches of mould, sometimes two or three inches in diameter, made their appearance. These were at first of a snowy whiteness, cottony, and dense, just like large tufts of cotton wool, of considerable expansion but of miniature elevation. They projected from the paper about a quarter of an inch. In the course of a few weeks the colour of the tufts became less pure, tinged with an ochraceous hue, and resembling wool rather than cotton, less beautiful to the eye or a lens, and more entangled. Soon after this darker patches made their appearance, smaller, dark olive, and mixed with, or close to, the woolly tufts; and ultimately similar spots of a dendritic character either succeeded the olive patches, or were independently formed. Finally little black balls, like small pin-heads, or grains of gunpowder, were found scattered about the damp spots. All this mouldy forest was more than six months under constant observation, and, during this period, was held sacred from the disturbing influences of the housemaid's broom, being consigned to the master's care with little compunction, but occasionally it became the subject of remarks not altogether flattering either to the wall or the moulds, or the master who was protector and patron of such a wretched mess.

Curiosity prompted us from the first to submit the mouldy denizens of the wall to the microscope, and this curiosity was increased week by week, on finding that none of the forms found vegetating on nearly two square yards of damp wall could be recognised as agreeing specifically with any described moulds with which we were acquainted. Here was a problem to be solved under the most favourable conditions, a forest of mould indoors, within a few yards of the fireside, growing quite naturally, and all strangers; could they all be related, or if not, why should all of them appear on that wall for the first time? Whence could these new forms proceed? Were they a new creation? Were they only other conditions of very common things? Certainly here was material for much reflection, perhaps some speculation. Some of the problems are still unsolved.

The cottony tufts of white mould which were the first to appear had an abundant mycelium, but the erect threads which sprung from this were all for some time sterile (Pl. LXVIII. fig. 1); they were slender, very delicate, jointed, and branched; so interlaced that it was difficult to trace the threads throughout their length, or to separate them from each other. Fertile threads were then developed in tufts mixed with the sterile threads, or individual fertile threads appeared amongst the sterile. These latter were rather shorter and stouter, also sparingly branched, but beset throughout nearly their whole

length with short, patent, alternate (mostly) branchlets. The branchlets were broadest towards the apex, so as to be almost clavate, and the extremity was beset with two or three short spicules (Pl. LXVIII. fig. 2). Each spicule was surmounted by an obovate spore ($a$) attached to the spicule by its smallest end (Pl. LXVIII. fig. 3). The presence of fertile threads gave the pale ochraceous tint to the tufts already alluded to. This tint was so slight that perhaps it would have passed unnoticed but for the proximity of the snow-white tufts of barren threads. The fertile flocci, it may be from the weight of the spores, were decumbent, hence the fertile tufts were not much elevated above the surface of the matrix.

This is a most interesting mould belonging to the order of *Mucedines*, but it seemed to agree so little with the characters of any known genus, that, on distributing specimens last year, it was placed provisionally in a new genus under the name of *Clinotrichum lanosum*; * since then, with the advice of some mycological friends, it has been referred to the old genus *Rhinotrichum*, as *Rhinotrichum lanosum*. Without entering here upon the reasons which led to this course, or attempting to discuss generic and specific distinctions, it is sufficient to indicate that the mould in question possessed such positive characters, and was so different from all recognised forms, that it not only had claims to be regarded as a distinct species, but it still remains doubtful whether it should not constitute the type of a new genus.

The mould above described having become established for a week or two, small blackish spots made their appearance on the paper, sometimes amongst thin patches of the mould and sometimes outside them. These spots, at first cloudy and indefinite, varied in size, but were usually less than a quarter of an inch in diameter. The varnish of the paper was afterwards pushed off in little translucent flakes or scales, an erect olivaceous mould appeared, and the patches extended to nearly an inch in diameter, maintaining an almost universal circular form.

This new mould sometimes possessed a dirty reddish tint, but was commonly dark olive. There could be no mistake about the genus to which this mould belonged; it had all the essential characters of *Penicillium*. Erect jointed threads, branched in the upper portion in a fasciculate manner, and bearing long beaded threads of spores, which formed a tassel-like head, at the apex of each fertile thread (Pl. LXVIII. fig. 4).

* CLINOTRICHUM, *gen. nov.* Hyphasma creeping; fertile flocci septate, decumbent, simple, or branched; branchlets alternate, patent, short, bearing at their tips a few spores attached to short spicules; spores simple. Type, *Clinotrichum lanosum.*—Cooke, *Fungi Brit. Exs.* No. 356.

For the benefit of the mycologist, we may observe that, although at first reminded of the *Penicillium olivaceum* of Corda by the colour of this species, it differs in the spores being oblong (Pl. LXVIII. fig. 4 B), instead of globose, and the ramifications of the flocci are different. Unable again to find a described species of *Penicillium* with which this new mould would agree, it was named *Penicillium chartarum.*

Almost simultaneously, or but shortly after the perfection of the spores of the *Penicillium*, other and very similar patches appeared, distinguished by the naked eye more particularly by their dendritic form (Pl. LXVIII. fig. 6). This peculiarity seemed to result from the dwarfed habit of the third fungus, since the varnish, though cracked and raised, was not cast off, but remained in small angular fragments, giving to the spots their dendritic appearance, the dark spores of the fungus protruding through the fissures. This same mould was also found in many cases growing in the same spots amongst *Penicillium chartarum*, but whether from the same mycelium could not be determined.

The distinguishing features of this fungus consist in an extensive mycelium of delicate threads, from which arise numerous erect branches, bearing at the apex dark brown opaque spores. Sometimes the branches are again shortly branched, but in the majority of instances are single. The spores are septate, sometimes with two, three, or four divisions, many of them again divided by cross septa in the longitudinal direction of the spore, so as to give a muriform appearance. As far as the structure and appearance of the spores are concerned, they are very similar to those of *Sporidesmium polymorphum*; consequently specimens were published as a variety of that species, but the accuracy of this determination is open to very grave doubts. The mycelium and erect threads are much too highly developed for a good species of *Sporidesmium*, and certainly so for the species to which they were referred, so that in the "Handbook of British Fungi" it is named *Sporidesmium alternaria*, for reasons hereafter detailed (Pl. LXVIII. fig. 7).

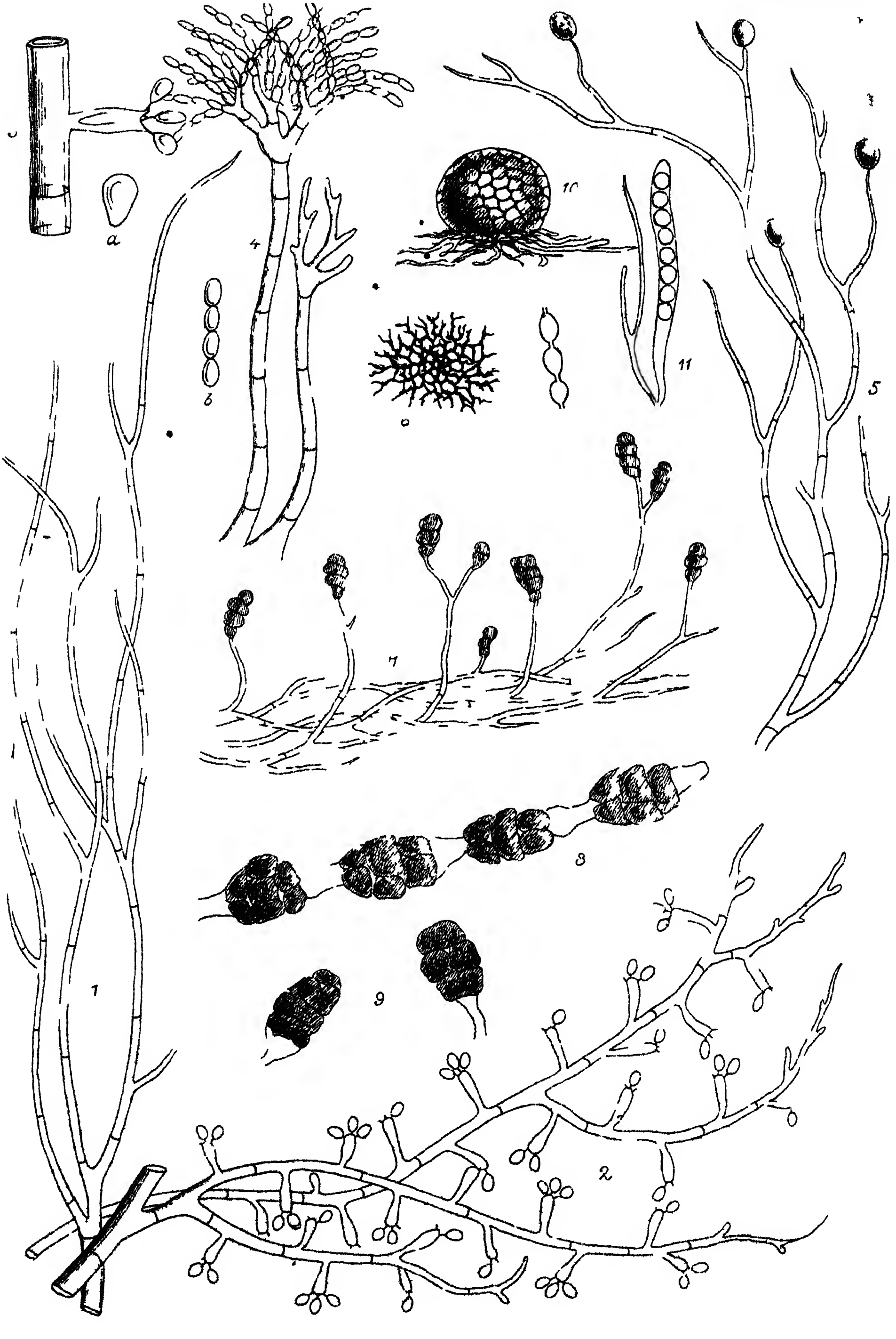
Preuss has described, in "Sturm's Flora," a species of *Alternaria* in which the spores are attached end to end in a beaded manner, as in other species of that genus, and the spores themselves are just of the character of the spores of our *Sporidesmium*, as will be seen by reference to the plate and comparison of figures 8 and 9. Preuss's *Alternaria*, which he calls *chartarum*, was also developed on paper, and it is not improbable that it is a more highly perfected form of the *Sporidesmium* in question. This view is strengthened by the appearance of freshly collected specimens of the *Sporidesmium*, in which, as

seen by a half-inch objective, the spores seem to be moniliform; but if so, the attachment is so slight that all attempts to see them so connected when separated from the matrix have failed. On one occasion a very immature condition of the *Sporidesmium* was examined containing simple beaded spores (Pl. LXVIII. fig. c) connected by a short neck. There is therefore some foundation for believing that the spores of this species are at first hyaline, simple, and connected together in a moniliform manner by a short apiculus; but, as subsequent search did not reveal any further corroborative evidence, it can only be considered probable. Finally, Mr. C. E. Broome, to whom specimens of the *Sporidesmium* were submitted, confirmed the observation that, when seen *in situ*, the spores seemed to be beaded.

The last production which made its appearance on our wall-paper burst through the varnish as little black spheres like grains of gunpowder. At first the varnish was elevated by pressure from beneath, then the film was broken, and the little blackish spheres appeared. These were, in the majority of instances, gregarious, but occasionally a few of the spheres appeared singly, or only two or three together. As the whole surface of the damp paper was covered by these different fungi, it was scarcely possible to regard any of them as isolated, or to declare that one was not connected with the mycelium of the others. The little spheres, when the paper was torn from the wall, were also growing from the under surface, flattened considerably by the pressure. We shall call this species, for the sake of distinction, *Sphæria cyclospora*. The spherical bodies, or perithecia, were seated on a plentiful colourless mycelium. The walls of the perithecia, rather more carbonaceous than membranaceous, are reticulated, bringing to mind the same structure in *Erysiphe*, to which the perithecia bear considerable resemblance. The ostiolum is so obscure that we could not be satisfied of its existence, or whether the perithecia are ruptured when mature. It is rather from analogy than positive evidence that the name of *Sphæria* is given (Pl. LXVIII. fig. 10). The interior of the perithecia is occupied by a gelatinous substance consisting of long cylindrical sacs or asci, each containing eight globose, colourless sporidia (Pl. LXVIII. fig. 11). These are accompanied by slender branched threads, called paraphyses, supposed to be abortive asci. At first, and for some time, the perithecia contain only a granular mass, at length mixed with paraphyses. The contents of the fertile asci are also at the first granular, and finally the sporidia are perfected.

We have now described, as fully as seemed to be necessary, the four forms of fungi which vegetated during last winter and

Plate LXVIII



spring on our damp wall. What presumption have we that they belong to one and the same fungus—direct evidence there is none—or should they be regarded each as autonomous? We have already intimated the difficulties which beset all attempts to obtain positive evidence in such cases. Already too many theories have been based on or supported by supposed results from the cultivation of fungi spores. Many ridiculous assertions have been made by those who have thus exhibited their thorough ignorance of even generic distinctions, to say nothing of more complex relations in mycologic science. Still we are by no means prepared to doubt that many of the recorded cases of polymorphism will ultimately be proved to be fact, and that many more will yet be discovered. We would admit that it is possible that none of the species, now included in the two great families of *Coniomycetes* and *Hyphomycetes* are autonomous. But, because it is *possible*, it by no means follows that we are prepared to condemn them by wholesale, or to admit that there is at present any evidence for doubting the autonomy of some entire genera. In the present condition of the study, and in the face of some startling facts, it is important that all observations should be recorded which bear upon the subject of polymorphism, whilst great care ought to be exercised in the declaration of positive judgment.

Reviewing the instances of association above recorded, and we should prefer, for the present, calling them *association* only, the mind naturally reverts to other and similar recorded instances. Supposing the whole of the four forms described above to be conditions of *Sphæria cyclospora*, there is no greater faith needed to believe it true than in the case of *Sphæria herbarum*. If *Alternaria tenuis* is really a condition of a *Sphæria*, why not *Alternaria chartarum*? If *Alternaria* be associated with *Cladosporium*, why not with *Penicillium*? Or if *Sporidesmium epochnum*, why not *Sporidesmium polymorphum*? And as for *Rhinotrichum* and *Penicillium*, it is just as possible for these to be polymorphic, as for *Dactylium*, *Dendryphium*, and *Verticillium*. When the presumption is confirmed by repetition, and more positive relations, there can be no doubt of a much more ready acceptance of their polymorphism than there would have been prior to the investigations of the Messrs. Tulasne and De Bary.

In this communication our remarks have related more particularly to the moulds, since attention has of late been directed to them in consequence of the supposed discoveries of certain German theorisers. It requires no little patience to comprehend the relations of Aërospores, Schizosporangia, Anäerospores, Aëroconidia, Thecaconidia, Anäeroconidia, and similar novelties, or to relish philosophy like the following recent importation:—

"Another most important vibrionic process—and which is as yet equally unknown to didactic science—is that of *massive vibrionic co-cementation*, as we find it, e.g. as a mass of 'matted' interlaced or entangled, and as it were 'coagulated' vibrios, e.g. in the 'mother of vinegar,' or 'phycomater:' forming a tough and apparently fibreless gelatine, uniform in all directions, and whence on the surface of the vinegar—as from the fetid scums of our hydrant-water likewise—little sprigs, like air-vesicles, become manifest, and that, rapidly enlarging in *bulk* and lengthwise, from their ramified fibre (and which *under* water readily *dissect into 'yeast' joints*) erect the blue tufted 'pencils'—of globular-beaded *single-file-spores* borne on fascicled joints—directly into the air. After being swelled by moisture, these beadlets become oval and, being exposed to the air, thick-coated. When such swelled and indurated spores of the blue pencil-tufts so abundantly found on *rotten apples, lemons, sweet potatoes, old cheese*, &c., drop off into the water or a fermentable *liquid*, they often directly enlarge into stemless, floating globular *seed-sporangia* (mucor) containing the blackish, globular seed within the leathery mucor vessel. But when such blue tuftlets ('Penicillium glaucum,' 'crustaceum,' &c.), e.g. as forming *floating islets* on old coffee and tea-decoctions, become entrained 'under water,' the beadlets become *confluent* by macerating their coats, and, under cover of an exuded floating scab, the whole vibrionic mass of liberated contents now constitutes a *polypous, wriggling pulp*—like crawling snods—and each constitutive vibrio (individually *coated* and forming a sort of short, *vibratile sprig*, called a 'bacterium') by individual lengthwise growth increases into a short automatously travelling and afterwards exceedingly long, but still as fine 'leptomitous' or individual fibre of the 'mephitic' water-rot of our pools and gutters"! * Such a lucid (?) explanation of polymorphic fungi as this extract contains can scarce fail to commend itself; the marvel *is* that the whole subject has not been cast aside by intelligent men as quite unworthy of serious attention.

We are unable, within the limits prescribed for this article, to explain the relations which subsist between such fungi as the "red-rust" and "mildew" of corn, and the barberry "cluster-cups;" or between the yellow rust and the black brand of the bramble and rose. In other words, the polymorphism of the *Uredines* and their allies. This is less to be regretted, since there has not, during the past four or five years, been any important additions to our knowledge on this subject, and what

* *"The Zymotic Fungus."* Experimental Investigations by Dr. T. C. Hilgard. St. Louis, Mo.

had previously been discovered and illustrated is very generally known.

If we are asked what deductions we are to make from the facts proved and the presumptions admitted, but not proven, we may answer briefly—that the tendency of recent discoveries, in the relations of one form to another amongst fungi, is to demonstrate that reproduction is not so simple a process in these low conditions of plant life as had hitherto been supposed. "This it is, and nothing more."

At one time the word "spore" represented the only recognised organ associated with the multiplication of fungi. Male organs or fecundative power was now and then mysteriously alluded to, but until recently all reproduction was supposed to be confined to a kind of germinative bud which was termed a spore. Each fungus was held to be perfect in itself, and reproduced itself, with no relation to any other individual, by this means. The change of opinion amongst mycologists is manifested, as much as anything, in the new terms, or the appropriation of terms from other cryptogams, now in vogue. Conidia, spermatia, oospores, zoospores, pycnidia, protospores, &c., all relate to organs but recently recognised in fungi. And, however much we may qualify the fact, however much we may doubt the evidence in special cases, we cannot ignore the conclusion that reproduction is very complicated, although very little understood in these extraordinary plants. Whilst admitting that the *Cladosporium* and the *Macrosporium*, the *Alternaria*, bottle-shaped cysts, and minute spermatia are all so intimately related with a certain species of *Sphæria*, that they can no longer be regarded as plants with a distinct autonomy and independent existence, we are unable to explain the relations which one bears to the other, or by what means each exerts its influence. The field for observation and research is a large one, and promises a rich reward; all that is required is earnest and careful workers, in which *this*, of all European countries professing to be scientific, has hitherto been most lamentably deficient. How long shall such a reproach continue?

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## EXPLANATION OF PLATE LXVIII.

FIG. 1. Barren thread of *Rhinotrichum lanosum*.
„ 2. Fertile thread of *Rhinotrichum lanosum*.
„ 3. Portion of fertile thread of *Rhinotrichum lanosum*, showing one of the branchlets with terminal spicules bearing the spores; A, a spore detached.

FIG. 4. *Penicillium chartarum*, considerably magnified; B, portion of chain of spores.
" 5. Fertile thread of *Mucor hyalinus*, bearing sporangia.
" 6. Dendritic patch of *Sporidesmium alternaria*, natural size, showing its habit.
" 7. Mode of growth of *Sporidesmium alternaria*, showing mycelium with erect threads and spores; C, supposed early condition of spores of the same.
" 8. Part of chain of spores of *Alternaria chartarum* of Preuss.
" 9. Spores of *Sporidesmium alternaria*.
" 10. Perithecium of *Sphæria cyclospora*.
" 11. Ascus with sporidia and paraphysis of *Sphæria cyclospora*.

# THE ECLIPSE EXPEDITIONS.

By RICHARD A. PROCTOR, B.A., F.R.A.S.
Author of "The Sun," "Other Worlds," "Saturn," &c.

BY the time these lines are read the results of the four expeditions which have been sent out from England to view the total eclipse of December 22, 1870, will probably be known throughout the greater part of Europe. To consider the probable nature of those results may therefore seem out of place and over-venturesome; while to discuss what preceded the setting forth of those expeditions may seem a waste of time, since nothing that can now be said, whether in the way of praise or censure, can affect the result. Yet it appears to me that this is the proper time and a suitable place for a brief discussion, both of the probable results of the eclipse expeditions, and of the circumstances which happened before those expeditions left our shores. As respects, in particular, the last of these subjects, considerations of the utmost moment to the scientific world, or at least to scientific Englishmen, are at issue. We are passing through a period of transition; and though there can be little question what will be the ultimate issue of the changes now in progress, though almost certainly a few years will place science in this country on a more satisfactory footing than at present, yet it is well to watch the signs of the times, to note the working of the old system, and to estimate rightly the great need there is of change.

Some eight months since, astronomers were beginning to urge the importance of making due preparation for the eclipse. It was felt that after what the American astronomers had done last year, England was bound to show her zeal in the cause of astronomy by sending parties to observe this European eclipse. Few expected at that time that the Americans would set competition (at least in this case) at defiance, by crossing the Atlantic in force, and doing here in Europe what we had not thought of doing last year in America. But it seemed clear to all that we were bound at least to observe our own eclipses—so to describe eclipses visible at European stations.

Now, at the very beginning, the impression was conveyed by those astronomers who are supposed to be officially connected with the Government, that it would be only as by an act of grace that Government aid would be granted. Those who were present at the meetings of the Astronomical Society, for example, when the subject of these expeditions was mooted, were painfully struck by the tone which pervaded the official communications addressed to that body. Astronomy appeared in the light of an importunate beggar about to renew her troublesome applications; while, though a hope was expressed that some assistance might be wrung from the Government, it was left to be clearly understood that the grant would be regarded as an act of great grace and condescension.

It is necessary, indeed, to point out that in all this a grave injustice was done to the Government. The real opinion of those in power was never really ascertained until much later. But whatever the cause may have been, certain it is that the impression conveyed during these preliminary discussions was, that Science—as represented for the nonce by Astronomy—had occasion to approach the powers that be in the garb of humility and self-abasement, to plead very earnestly if she would gain a small modicum of help, and to be well content with whatever the Government might be disposed to give her.

This, let us proclaim it at once and loudly, is *not* the proper attitude for Science. If she must needs come as an applicant, she should come as the Sibyl of old, giving clear intimation that what she offers is worth more, a hundred-fold, than what she demands. She should come as conscious that what she asks is to be the benefactress of the human race. To quote Professor Tyndall's noble words:—"Science does not need the protection of men in power, but desires their friendship on honourable terms. By continuing to decline the offered hand, they will be invoking a contest which can have but one result. Science must grow. Its development is as necessary and as irresistible as the flowing of the tides, or the motion of the Gulf Stream. It is a phase of the energy of Nature, and as such is sure, in due time, to compel the recognition, if not to win the alliance, of those who now decry its influence and discourage its advance."

But so timid were the men of science to whom the task of approaching the Government fell, that their voice was for many weeks—invaluable weeks—altogether unheard. It pleased them in the first place to apply to the wrong department, and in the wrong way. Certainly the mode of application was that which had been adopted in former years; but there have of late been changes, and the old mode of application was as little likely to be effective as the plan of posting

letters (in the first convenient chink) adopted by Mrs. Tadgers's old woman-of-all-work. Nearly fifty days elapsed, during which no reply whatever was received; and then a reply came which implied—as far as this wrong department was concerned—a flat refusal.

If this result had been made public at once, it is probable, or rather, it is certain from what actually followed, that the greater part of the mischief might have been remedied. But the old fear of giving offence to those in power appears to have operated, and only gradually, and in an unofficial manner, the public learned that (as it seemed) Government had withdrawn the light of its countenance from the science of our day. Then the natural result followed. The public press—the *Daily News* leading the way—appealed loudly against the supposed decision of the Government. To the public press, and to the public press alone, it was owing that Government changed their mind, if even it was not through the public press alone that Government first really heard what was required of it. Then one would have supposed that the committee of scientific men appointed to deal with the matter would have sent in appeal after appeal until the required assistance was obtained. But something quite different took place. Those who managed the committee waited until Government itself intimated its readiness to consider the proposals of astronomers. Even then the committee were in no haste to act. "As quickly as their constitution permitted," to use the euphemistic expressions of the Astronomer Royal, or, in other words, after wasting a fortnight of most valuable time, they sent in a new application; and this they did so gently that when, a week later, Mr. Lockyer applied directly to the Chancellor of the Exchequer, he found Mr. Lowe in complete ignorance that anything was wanted from the Government.

It was now the second week in November; the eclipse some six weeks off. The sum of two thousand pounds and the means of transport had been promised by the Government, but it seemed likely that the grant would be of little use, since so short a period remained for organisation. Six months had dwindled down to six weeks. Not only had the actual work of organisation been thus left to be completed in a space of time altogether inadequate, but those who had volunteered to join the expedition, and who had all this time been in doubt whether their services would be required or not, had now little time left to prepare themselves for the work they desired to perform.*

* I will mention one instance out of many that I know of to illustrate the mischievous effects of the delay. Mr. W. H. H. Hudson, M.A., a Fellow of St. John's, and a very skilful mathematician, had proposed to study the

As to the work performed by the organising committee in the brief interval which now remained, I do not care to say much. That there were shortcomings cannot be denied. Contradictory directions were sent by post and telegraph.* Eminent men of science, learned Fellows of colleges, and disinterested volunteers in the cause of astronomy, received telegrams so curt, and even impertinent in tone, as to have justified their withdrawing wholly from the work they had volunteered to do.† But then it must be remembered that the work of months was being crowded into weeks, and that a large part of the blame should in justice be removed from the inferior officers who had to superintend this part of the work, since it was certainly not their fault that the time at their disposal was so limited.

To sum up this more painful part of my subject, there was complete mismanagement from beginning to end. To whom we should ascribe the blame of the *fiasco* (for let the success of the expeditions themselves be what it may, the preparations were a complete *fiasco*) it would be difficult to say. We cannot rightly place the blame on the shoulders of the Astronomer Royal, whose official duties at Greenwich would have justified him in leaving the matter wholly to others. And again, the names of many in the committee—as General Sabine, Messrs. Lassell and De la Rue, Colonel Strange, Dr. Huggins, and so on—are guarantees of an earnest regard for scientific interests. And as for the rank and file of the committee, if I may be permitted the expression, it is well known that the matter was taken completely out of their hands. But *somewhere* there

corona with the polariscope. In order to do this the more effectually, he had intended, if the expeditions were decided on, to devote a large portion of the long vacation to making himself practically familiar with polariscopic analysis. He received definite intimation that his services would be accepted on Nov. 22; that is, in the heart of the October term (the busiest term of all at Cambridge), when his whole time was taken up with lectures and the work of preparing questionists for the tripos of the present month.

It is not too much to say that the actual efficiency of the observing parties has been reduced by much more than one-half, through the delay which resulted from the "weakly constitution of the committee" appointed to manage matters.

* I was myself present when Mr. Brothers, of Manchester, received within a few minutes three contradictory sets of instructions, one by post and two by telegraph.

† In one instance, a Fellow of a college received a telegram so remarkable in tone, from a member of the organising committee, as to be obliged to intimate that even the curtness of telegram-English would not justify the rudeness he had been subjected to. It is only right to add, however, that the courtesy of the honorary secretary of the committee was favourably commented upon by all who received communications from him.

must have been a leaven of undue subservience to the powers that be, and unfortunately this leaven "leavened the whole lump."

Let us turn to a pleasanter subject.

The course of the moon's shadow during the eclipse is somewhat remarkably curved, so that, though crossing the southern part of the Spanish Peninsula towards the west, and Sicily and the South of Turkey towards the east, it yet dips southward into Algeria and Tunis. The expeditions have been so planned as to take advantage of this peculiarity. The weather in Spain is not likely to resemble that in Sicily; while in Algeria it is probable that a totally different condition of weather may prevail than at any of the European stations.

The chances of the parties at Cadiz and Gibraltar are probably about equal.

The Cadiz party is under the charge of the Rev. Fr. Perry, S.J. Here three classes of observation are to be made. There will be spectroscopic observations of the corona, by Fr. Perry, assisted by Mr. Hostage and by Mr. Abbay. The polariscopic observations will be made by Mr. Hudson, M.A., Fellow of St. John's College, Cambridge, and by Mr. Moulton, B.A., of Trinity. Both these gentlemen are skilful mathematicians, and familiar with the theory of polariscopic analysis, which indeed forms a part of the Cambridge mathematical course. Sketches of the corona are to be made by Messrs. Naftel, Smyth, Penrose, and Collins; while Captain Toynbee superintends the chronometric arrangements.

The Gibraltar party is in charge of Captain Parsons. Spectroscopic observations will be made by Messrs. Carpmael and Gordon. Messrs. Lewis and Ladd superintend the polariscopic work. Mr. Hunter and two Oxford undergraduates will sketch the corona. But at this station other observations are to be made. The planet Saturn will be close by the sun, and Messrs. Talmage and Maclear propose to examine very carefully the appearance presented by the planet under these circumstances. Professor Thorpe, formerly of Owen's College, Manchester, and now of Glasgow, will study the changes in the chemical activity of the sun's light during the eclipse: and, if possible, during totality, he will endeavour to estimate the quality of the corona's light in this respect. Lastly, Mr. Buckingham, assisted by Mr. Spiller, will apply a very powerful telescope to obtain photographs of the eclipsed sun.

The only other European party is that which is to view the eclipse from the neighbourhood of Syracuse. This party is a remarkably large one. It is in charge of Mr. Lockyer. Several series of spectroscopic observations are to be made. Professor Roscoe, assisted by Mr. Bowen, will conduct one series; Mr.

Lockyer, assisted by Mrs. Lockyer, a second; Mr. Seabroke, assisted by Mr. Burton, a third; and Mr. Pedlar a fourth. Messrs. Ranyard, Griffith, and Clifford will superintend the polariscopic observations; Messrs. Brett and Darwin will make sketches of the corona; while Messrs. Vignolles, father and son, will superintend the chronometric arrangements, and make general observations. But, probably, the most important work done at this station—if the weather is favourable—will be that superintended by Mr. Brothers, one of our most skilful photographers. Assisted by Dr. Vogel and Mr. Fryer, he hopes to obtain two series of views, one by means of one of the Sheepshanks' equatorials, belonging to the Royal Astronomical Society; the other by means of a photographic camera of his own.

I have kept to the last the strongest party of all; that, namely, which, under the charge of Dr. Huggins, proceeds to Oran, in Algeria. Here the duration of totality will be only three seconds less than the actual maximum. The name of Dr. Huggins is alone a guarantee that the spectroscopic study of the corona will not only be conducted skilfully, but with a most careful reference to strict scientific principles. He is in alliance, however, with other eminent physicists. Professor Tyndall and Dr. Gladstone are with him. Mr. Crookes joins in the spectroscopic work. Captain Noble and the Rev. F. Howlett will see that proper portions of the corona are brought upon the slit, while these two practised observers will have at the same time the opportunity of viewing the image of the corona on the screen in which the slit is made. This screen is covered with rectangular cross-lines, and the true shape of the corona will thus admit of being very readily noted. All the arrangements for viewing the spectrum of the corona and recording the place of any lines which may appear have been superintended by Dr. Huggins, at whose house I had the pleasure of inspecting them thoroughly. I cannot doubt that the actual observations—to be made severally by Dr. Gladstone and Mr. Crookes—will be successful, if the weather only be favourable. Dr. Huggins himself—after seeing before totality begins that the adjustments are properly made—will devote his attention to the telescopic study of the corona. I have dwelt so much on the importance of keeping the eyes in darkness for a few minutes, at least, before totality begins, that I need hardly remark how well pleased I have been to find that so eminent a physicist and so skilful an observer intends to adopt this precaution. I attach very great importance to this feature of Dr. Huggins's plan; since very little could, I think, have been expected from the mere renewal during this eclipse of observations which have been made repeatedly during former eclipses of greater extent and importance.

The polariscopic observations at Oran are to be made by Mr. Carpenter, of the Greenwich Observatory.

As regards the results which are to be expected from these four expeditions, supposing the weather to be favourable, it does not seem to me difficult to form an opinion.

In the first place, it must not be concealed that, in this as in all former eclipse expeditions, no inconsiderable proportion of the suggested observations are likely to be of no practical utility whatever. For example, there can be no question that all the observations which are directed solely to determine whether the corona is a solar appendage will simply involve a waste of labour. It has been a misfortune that any doubts should have been started respecting a matter so thoroughly demonstrated; but this misfortune it is now too late to remedy. Nor must we forget that in former instances an even larger proportion of observing energy has been thrown away. For when, in 1860, not only England but France, Italy, Germany, and other countries sent forth their astronomers to view the Spanish eclipse, the doubts which Faye and others had urged respecting the reality of the prominences influenced more than nine-tenths of the observers. Nearly a hundred astronomers and observers endeavoured to find out whether the prominences are real solar phenomena, or mere illusions—lunar mirages, perhaps, as Faye had suggested; and it would be difficult, indeed, to say how much knowledge which, but for these ill-considered doubts, might have been acquired, was thrown away on that inauspicious occasion. The success of De la Rue and Secchi in photographing the eclipsed sun does indeed serve to render the eclipse observations of 1860 memorable, and in a sense to hide from our view the real failure of astronomers at that time. But the very success of the two who chose to work independently and usefully, only causes us to deplore the more that thirty times as many preferred to waste their energies in demonstrating the demonstrated.

On the present occasion, however, those who have pleaded for useful observations have not been wholly unsuccessful. A relatively small proportion of observing energy is to be devoted to demonstrate the abundantly demonstrated fact that the corona is a solar appendage.* Nearly all the most skilful telescopists and spectroscopists propose to inquire what the actual constitution of the corona may be, regarding its position—very

* I am told that, in a recent number of "Nature," it is remarked in a leader that, "despite some hard writing to the contrary, the position of the corona remains to be proved." This is in a sense true; and so, also, it is true that every proposition of Euclid (as, for instance, Prop. 5, Book I.) remains to be proved, *as far as some learners are concerned.*

justly—as already established. The influence of Faye's doubts about the corona has been far less than that of his doubts about the prominences in 1860. In the plan of operations proposed for Dr. Huggins's party, in particular, one can trace no signs of any evil influence exerted by these doubts. Every suggested observation is such as will *tell.* The spectroscopic observations will either reveal new bright-lines in the coronal spectrum, or exhibit the Frauenhofer lines, or else prove that the spectrum really presents no other features than were seen by the American observers, Young and Pickering. The operations of Captain Noble and the Rev. F. Howlett, as auxiliaries in these observations, will be especially valuable; for we shall not only have unexceptionable evidence as to the parts of the corona actually analysed, but also full information as to the figure of the parts examined; and this, combined with Dr. Huggins's study of the structure of the corona, as seen in the telescope, can hardly fail to lead to results of the utmost interest and significance.

I should be led to attach almost equal importance to the telescopic and spectroscopic observations to be made by Mr. Lockyer's party, were it not for the unfortunate doubts which Mr. Lockyer himself entertains respecting the corona's position. One cannot fail to recognise in his instructions the effects of these doubts; the suggested observations seeming to have scarcely any other end than to solve them. There is also another strange opinion of Mr. Lockyer's, which seems almost certain to exercise an unsatisfactory influence upon the observations made by his party. He holds the chromosphere as seen by aid of the spectroscope to be only the lower portion of an envelope extending, in reality, far above the highest of the prominences. This seems to me a strange delusion. No one familiar with the history of former total eclipses can fail to recognise the fact that the outline of the chromosphere—or, as the discoverers of the layer called it, the *sierra*—is as well defined as that of the prominences. There is indeed not a particle of evidence tending to the belief that in eclipses it would appear otherwise than in the valuable drawings which Professor Respighi has obtained by aid of the spectroscope. Indeed, I might go much farther, and say that everything we know of the chromosphere points to the belief that it is not a solar atmosphere at all, as has been assumed, but is formed rather of small prominences and of the remains of those loftier prominences which Zöllner and Respighi have watched sinking back towards the solar surface. The gaseous envelope into which these gaseous prominences are projected to vast heights, and through which they sink slowly back, is doubtless to be regarded as the true solar atmosphere, and not that glowing

and somewhat ruddy layer whose serrated surface may be seen far below the sinking prominence-matter.

It is very probable, however, that the attempt which Mr. Brothers proposes to make to secure photographs of the corona, may cause the Sicilian party to be one of the most successful. His plan is to take a double series of photographs, one with a Sheepshanks' equatorial belonging to the Astronomical Society, the other with a camera of his own, mounted upon the equatorial and carried by the same movement. The camera pictures are intended to include a wide field, and it is far from improbable that the long coronal beams may thus, for the first time, be rendered visible in a photograph. I have had the advantage of a full discussion with Mr. Brothers of the plan he proposes to adopt, and I quite concur with him in thinking that, if weather alone be favourable, his operations are likely to be rewarded with a fuller degree of success than has yet rewarded attempts to photograph the corona.

With regard to the long beams, I may remark that I regard them as among the most remarkable and significant phenomena presented to us by the corona. If the pictures which have been drawn by Gilman and others during recent eclipses be accepted as indicating the exact proportions of the dark and bright beams, a problem of great difficulty is presented to us. We might readily be misled by these radial beams to regard the corona as due to the passage of light-rays between the inequalities of the moon's surface, did we not attend to certain considerations which negative such a theory. Fr. Secchi, indeed, compares the corona to what is seen when the sun's light is admitted into a darkened room through a nearly circular opening imperfectly stopped by a circular plug, or through a circular opening imperfectly stopped by a nearly circular plug. Dr. Oudemann, also, has put forward a somewhat similar interpretation of the coronal beams,* which he supposes due to the illumination of matter lying between the moon and earth.

When such views are studied carefully for awhile, however, fatal objections become apparent. Let us suppose for a moment—in order to give Oudemann's theory a chance, so to speak—that we may neglect that matter of the same sort which lies beyond the moon, and which, being illuminated more

* Dr. Oudemann's theory has lately been brought by Dr. De la Rue under the notice of the Astronomical Society, but was not received very favourably. Of late, indeed, Dr. De la Rue has shown, as respects such theories, *a consideration for the weak* which appeals strongly to our sympathies as Englishmen, however far it may be from commanding our agreement as students of astronomy.

brilliantly than the matter on this side of the moon, and presenting also a far greater depth of illuminated matter, ought to give nearly all the light seen close by the moon—let us, I say, neglect this consideration, and deal only with the matter lying between the moon and earth. Then the sun's rays, passing the rough edge of the moon and falling on this matter, would certainly produce a radial appearance, resembling very closely the corona as pictured by eclipse-observers. There would be bright radial beams and intervening narrow spaces, precisely as in the picture by Mr. W. S. Gilman, jun., in Commodore Sands' report of the American eclipse. But in order that these radial beams should extend as far as they have been actually seen during eclipses, the matter capable of being thus illuminated should extend (one may easily calculate) fully 200,000 miles from the moon towards the earth. Now, assuming with Oudemann, that this matter is the exterior part of the Zodiacal Light, there is no reason why it should not extend as far as this towards the earth, or very much farther. But, then, as the corona has been seen as well in June and July as in December and January, that is, as well when the earth is a million and a half miles beyond her mean distance as when she is as much within that distance, we are utterly prevented from supposing that the limits of this zodiacal matter lie always somewhere between the moon and earth, separated as these bodies are by less than a quarter of a million of miles. In fact, to account for the visibility of the corona in all total eclipses, we must assume that when the earth is in perihelion, the zodiacal matter extends three millions of miles or so (at least) beyond the earth. This matter, thus extending beyond the earth, ought to be visible at night in a far more conspicuous manner than the corona during totality. For, according to the theory, those particles within the quarter of a million of miles separating us from the moon which are but obliquely illuminated, and whose brilliancy is marred by the strong light continuing during even the most considerable total eclipse, are yet so conspicuously lighted up as to show the radial beams on a bright background. How much more conspicuous then should be the illumination seen towards the south at midnight, where (according to the theory) a space some three millions of miles deep, full of this matter, is illuminated directly—or as the full moon is—while the darkness of night and the black background of the sky help to render the phenomenon more conspicuous. The black shadow of the earth would indeed be thrown as a long black rift across this illuminated region of the heavens; but we know how far it would reach, and what its shape would be. Even at the moon's distance the true shadow would be but about three times the moon's diameter in breadth, while at a

distance of 900,000 miles, or more than 2,000,000 miles from the imagined limits of this zodiacal matter, the shadow would end in a point. Nor would the penumbra extend far enough to hide any but a relatively small portion of the supposed matter.

It is hardly necessary to point out that no such phenomenon has ever been witnessed either in the winter months or at any other season. So that, independently of a host of other objections, this one—rightly understood—disposes of Oudemann's theory, and of all others which require that matter admitting of being rendered visible to us by the sun's light exists at the moon's distance.

Yet these radial beams remain to be explained, for I think few will be disposed to assert that they are due to mere optical illusion.*

I believe that the chief interest of the eclipse observations is not unlikely to be associated with the interpretation of the coronal radiations. For, as it seems to me, the difficulty of interpreting them is altogether greater than that of explaining the corona itself. As respects this last, indeed, it seems to me improbable that the evidence we have can be made much fuller or more convincing than it is at present. But as respects the beams we have much to learn before it would be safe to hazard an opinion. Nor is it by any means unlikely that we may find in these beams a problem as difficult of solution as that presented by the phenomena of comets.

It will be well to consider some of the accounts which have been given of the coronal beams. More particularly it will be interesting to inquire whether we have satisfactory evidence as to their fixity during the whole continuance of totality. For though their appearing to change in position would afford us very satisfactory evidence as to their nature, their immobility, if it could be established, would have great significance.

It is worthy of notice, at the outset, that accounts referring to apparent motion are less likely to be trustworthy than those which distinctly state that the beams remain fixed in position. For, in the first place, an inexperienced observer might very well be misled into the supposition that a radiated glory of light had a certain degree of motion; and in the

* Oudemann accepts somewhat confidently Dr. Gould's statement that the coronal beams changed in position during the American eclipse. Dr. Curtis remarks however, and the study of the various narratives fully confirms the assertion, that all the other observers describe the rays as fixed in position. I do not know that their apparent movement, if confirmed, could be regarded as demonstrating anything as to their nature; but in this, as in other cases, all the best authenticated accounts speak of them as remaining unchanged.

second, the shifting lights around the horizon, as the shadow sweeps across the station of the observer, would tend greatly to encourage the illusion. To this may be added the circumstance that at the first formation of the corona, and as it is disappearing, an apparent rotational movement results from the rapid closing in and separation of the solar cusps. I find that the accounts of apparent motion in the coronal beams are few in number and of little weight compared with those which assert the fixity of the rays; and I cannot recall any instance in which an observer speaks of the apparent motion as of a phenomenon he had been at the pains to convince himself of, whereas those who refer to the fixity of the beams speak sometimes very definitely on this point.

Here are a few accounts of apparent motion.

Don Antonio d'Ulloa, speaking of the eclipse of 1778, says, "the corona seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre." But it does not seem at all clear that he refers to the beams, because he says, "there appeared issuing from the corona a great number of rays of unequal length, which could be discerned to a distance equal to the lunar diameter." This "discerning" of the rays seems to imply that they were unmoved; and certainly the observation would prove too much if it were accepted as establishing a sort of Catherine-wheel motion of the radial beams.

In 1842 several observers, says Grant, "asserted that the ring of light" (not the rays) "turned continually round its centre." "At Lipetsk," he adds, "the light of the ring seemed to M. Otto Struve to be in a state of violent agitation." Then follows the best testimony yet given in favour of apparent changes in the beams. "Mr. Baily states," says Professor Grant, "that the rays had a flickering appearance, somewhat like that which a gas-illumination might be supposed to assume if formed into a similar shape." It will be admitted, however, that there is here no convincing evidence of a change of *place* in the beams, and further that the changes of brightness are fairly comparable with those flickerings which Chladni, Encke, Humboldt, Bessel, and other astronomers have noted in the case of comets, and which have even at times been noticed in the zodiacal light. When we remember that the coronal beams are necessarily seen *through* our atmosphere, which must undergo very important changes of temperature during the continuance of totality, and probably be subject to waves of disturbance, we cannot wonder that the illumination of these delicate objects should seem fitful.

Now when we turn to narratives describing the fixity of the coronal beams, we find much more satisfactory evidence.

In the carefully observed eclipse of 1733, M. Edstrom, mathematical lecturer in the Academy of Charlestadt, noted "that the ring appeared everywhere of equal breadth, save where it emitted rays from above as well as from below; that these rays were equal in brilliancy, but unequal in length; and that *they plainly maintained the same position, until they vanished along with the ring upon the appearance of the sun's limb*." I take this account from Professor Grant's admirable "Physical History of Astronomy;" and the italics are his. The same eclipse afforded abundant evidence of the extreme delicacy of the light of these radiations, for "at Lincopia," says Grant, "the ring appeared of a bright white colour, but *it did not exhibit a radial aspect*."

Bruhns, of Leipsic, speaks thus of the appearance of a long radial beam seen during the eclipse of 1860: "On the eastern side a long ray shot out to a distance of about a degree; at the base its breadth was three minutes, but it tapered down to about a minute and a half in breadth near its extremity. *During the ten seconds that my attention was directed to it, neither the direction nor the length of the ray altered.* Its light was fainter than that of the corona, which was brilliantly white and seemed to twinkle."

I have already remarked that all the observers of the American eclipse of last year, save Dr. Gould alone, speak of the fixity of the beams. The following instances may be specially cited. Professor Simon Newcombe says: "Looking directly at the corona, there was no actual appearance of striation, but it seemed to be of a jagged outline, extending out into four sharp points nearly in the horizontal and vertical direction, while between these points the serrated edge hardly seemed to extend beyond the body of the moon. The greatest distance to which the extreme points seemed to extend did not exceed a semi-diameter of the moon, and there was nothing like long rays of light extending out in any direction whatever. *When I turned my head the points did not seem to turn with it.* Still I experienced a singular difficulty in judging accurately either of the number or direction of the jagged points, or of the extent to which they might be optical illusions produced by the differences in the height and brilliancy of different parts of the corona. . . . Seen through a green glass, the corona consisted simply of four or five prominences, extending around the moon, smooth in their outline, shading off by imperceptible gradations, and rising to different heights."

General Myer, by observing the eclipse from the summit of White Top Mountain, obtained a far better view of the radial beams than any of the observers at lower levels. "As a centre," he says, "there stood the full and intensely black

disc of the moon, surrounded by the aureola of a soft, bright light, through which shot out, as if from the circumference of the moon, straight, massive, silvery rays, seeming distinct and separate from each other, to a distance of two or three diameters of the lunar disc, the whole spectacle showing as upon a background of diffused rose-coloured light. . . . The silvery rays were longest and most prominent at four points of the circumference, two upon the upper and two upon the lower portion, apparently equidistant from each other, giving the spectacle a quadrilateral shape. *The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the compass. . . . There was no motion of the rays.*"

The spectroscope may perhaps afford some information respecting the structure of these beams; but the faintness of their light will render spectroscopic observation very difficult. If I were willing to hazard a speculation as to their structure and physical cause, I should associate them, I think, with the tails of comets, and regard them as phenomena indicating the action of some repulsive force exerted by the sun. Sir John Herschel has pointed out that we have demonstrative evidence of the real existence of repulsive forces exerted by the sun with great energy under certain conditions and upon certain forms of matter. A source of perplexity exists, however, in the relative narrowness of these beams, whose apparent cross-section, as delineated by most observers, is far less than the apparent diameter of the sun. One would thus be led to infer that the real seat of these repulsive energies lies far beneath the solar photosphere. It is worthy of notice, too, that the beams usually appear to extend from the zone of spots; and one might almost infer that the repulsive action is exerted with peculiar energy in lines extending from the sun's centre towards the so-called spot-zone.

It is with reference to such questions as these that the observation of the present and future solar eclipses is so full of interest. There are problems presented by the corona which are as yet not only unsolved, but apparently very far from solution. All the energies of our observers need to be directed towards the mastery of these difficulties; and therefore it is that, as I think, I have been right in urging as earnestly as possible that the records of *past* eclipses should be made to bear fruit in the interpretation of all problems which can be interpreted by means of them, while the opportunities afforded by *future* eclipses, and the skill of those who observe them, should be wholly devoted to those more perplexing problems which still await even the means of solution.

The news has just arrived that the Sicilian party will be dis-

tributed among four stations—one party will be at Syracuse, another at Augusta, a third (the head-quarters) at Catania, and a fourth, a strong party headed by Professor Roscoe, at the summit of Etna. Although this arrangement is associated in the telegram with the accident which happened to the Psyche (the news of which sent a thrill of alarm and anxiety through the hearts of all who take interest in the cause of science), I am disposed to hope that some parts of the arrangement may be referred to other considerations. The fact that a portion of the expedition is able to travel so far south as Syracuse, actually crossing the line of central eclipse, seems to imply that the expedition has not been seriously hampered by the accident. I hope much from the party at Etna. I had dwelt earnestly on the advisability of sending a party there (see the December number of "Fraser's Magazine"), and I think it cannot be doubted that, though the totality will last there but a short time, more instructive observations of the corona will be possible at so considerable an elevation than any which have yet been made.

# NOTES ON BUTTERFLIES.

By the Rev. C. HOPE-ROBERTSON, F.R.M.S.

[PLATE LXIX.]

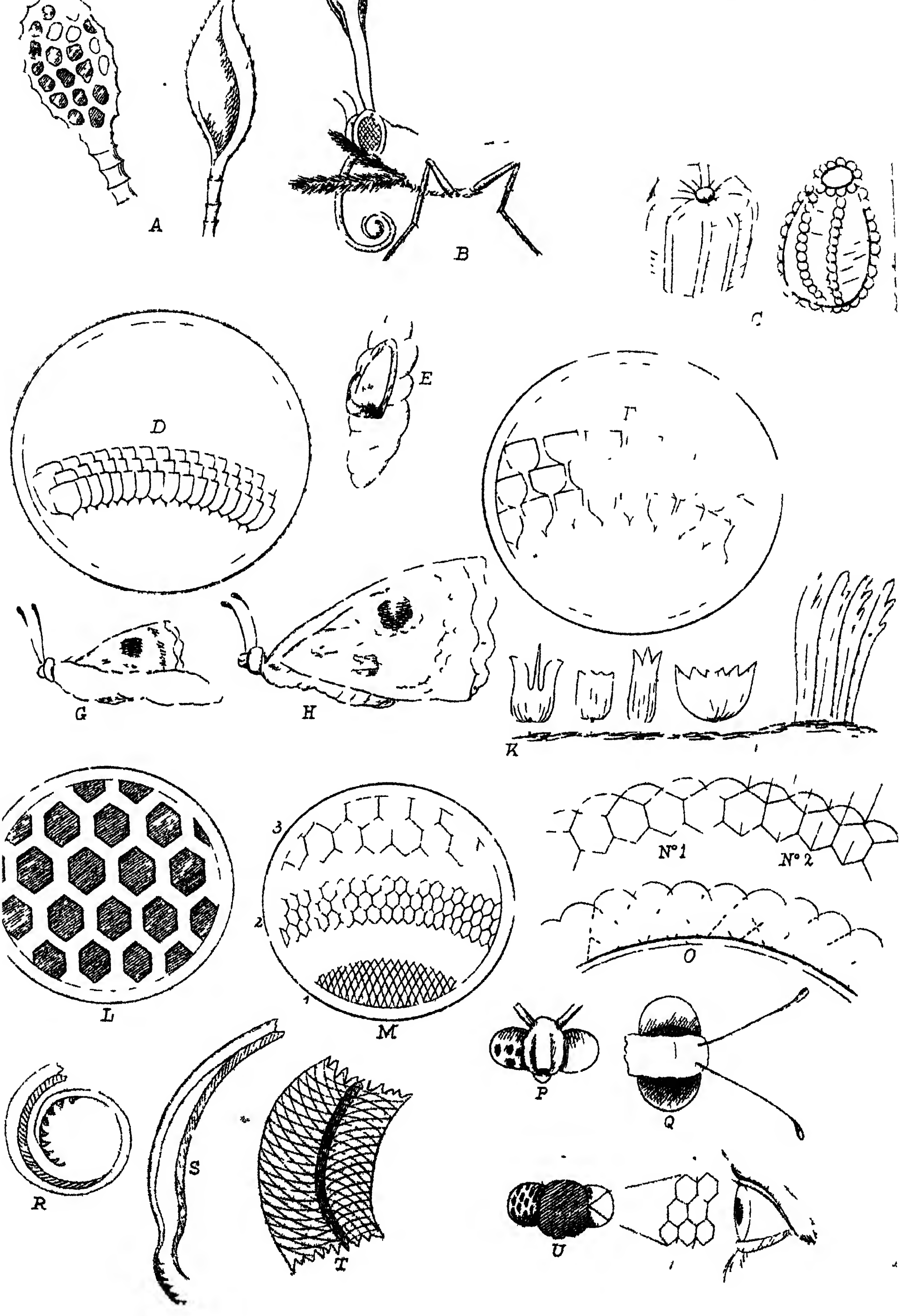
THESE lovely creatures give as much pleasure to the student of science as to the child at play, who runs, cap in hand, to chase them. Their colours, forms, and habits, all furnish charming subjects for observation. We propose to collect some notes taken at intervals on these points, and show the methods available for easily exhibiting their interesting peculiarities.

We begin with the distinction which serves, in general, to divide butterflies from moths, which is the existence of a *club-shaped* end to the antennæ of the butterfly; while the moth has sharp-pointed or plumed antennæ. From this club-shaped end comes the generic word *Rhopalocera*, compounded of the two Greek words for a *club* and *horn*. Observation has found this distinction characteristic of the butterfly tribe; we wish further to see what there is in the habits of the creatures to explain the reason why they have such horns.

If we look at one on a bright sunny day we will see its horns erect, stretched out, delighting in the sunshine. While, if we look at a moth, which flies chiefly at dusky hours, when it is brought to the light its horns are folded back, and doubled as far as possible under shade of its body or wings, as if unable to bear the glare. They are evidently most sensitive to light.

Now this is a key to one purpose which the horns serve. They are the thermometers of these little weather-watchers. The degree of warmth needed for them is made known by the effect of light on the delicate tips of their horns or feelers. A club-shaped feeler gives a larger surface for the light to act upon; and the nervous energy needed for the butterfly to enable its muscles to work, is collected by the club-shaped points, and its general influence stored up to sustain flight against the effect of a chill. Cold would paralyse them. Now if we take the microscope to examine the structure of the club-points, these are found to be full of cells, or depressions, where the heat and electric energy, accompanying light, may be

Plate LXIX



collected and transmitted to the system for use. Some of these are figured at A, Plate LXIX.

The butterfly tribe is one of the order *Lepidoptera*, or scale-winged insects, so called from the multitude of little scales with which the wings are covered.

These are of various forms, colours, and textures, sufficiently distinct to mark the species, with a wonderful variety on the different parts of the wing. The shapes are clear and compact on all the body of the wing; while at the edges there are longer shapes, like plumes, or the wing feathers of birds. They are attached, each by a separate stalk, to the membrane of the wing. Some of these diversified forms are figured at K. They come off like dust when slightly rubbed. The lovely colours of these scales seem to depend, not always on an actual sheet of local colour, but to be due sometimes to the *dispersion* of light by means of fine lines or striæ drawn on each scale. The relation of the size of lines, closely ruled, to the length of a wave of light, is, by the undulatory theory, easily shown to account for the dispersion of colours by a ruled surface, in itself colourless. Buttons have been made, as curiosities, thus artificially ruled with very fine lines, giving a beautiful play of changing colour. The colours of mother-of-pearl, and other substances, seem due also to the dispersion of light on a white ground. Now, in the case of the purple Emperor a changing play of colour is seen, between brown and purple, as it catches the light at different angles. This appearance can be calculated as due to the relative breadths of the lines covering the scales (which vary from 10,000 to 30,000 in the inch), when compared with the difference between the lengths of waves of light.

These lines on the scales run always from end to end, never across. The reason for this seems to be that by this arrangement the drops of rain, if falling on the wing, are rolled off easily. The wing is nearly *waterproof*; single drops running off very cleanly, leaving no trace, if the scales are uninjured.

The arrangement in position on the membrane of the wing is a curious point about the scales, and explains the wonderful expansion of the wing, during the first half-hour after the butterfly comes out of its chrysalis. When first emerged the comparative size of the wing to the body is seen at fig. G. In about half-an-hour it will be seen to have expanded to about the size figured at H.

To find the rationale of this development, we must take a chrysalis when nearly full grown, and gently lift off the skin covering the future wing, whose outline is easily traced on the side of the chrysalis, as at E. Applying a microscope, we can see the scales very closely packed together, slipped under each

other both lengthways and sideways, as at fig. D. When the full development of the wing is attained, the scales are then to be seen in a much more expanded state, spread out from each other in all directions, as at fig. F. The membrane of the wing, to which the roots of the scales are fixed, is elastic when soft. In this state it emerges from the chrysalis, and, air being breathed by the air-vessels, is sent to the extreme edges of the wing, by many air-tubes which pervade the skin; the air stretches these tubes, which in turn expand the skin, and this pulls out from under each other the different rows of scales, until they assume a position in order, like the slates of a housetop. By this time the skin gets hard, and when once dried is stiff enough to remain in the flat shape required for future flight. But till this is done the insect is helpless, and many a one is then picked up as a choice morsel by quick-eyed birds.

The colours of the wing in its moist, fresh, dustless purity are surpassingly lovely. Nothing can be more attractive as a sight than the flight of a dozen or so of new-fledged Peacock butterflies about one's breakfast-table. They may easily be kept thus to come out in succession by securing some dozens of the black-looking caterpillars, off a bed of nettles, where they feed. A large box, covered with wire gauze, to keep out wasps and ichneumon flies, may be their home. Fresh nettles must be daily given them. And then, by degrees, they all go into the trance-like chrysalis stage, hanging to the sides or top of the box. Each morning after they begin to come out a fresh batch will be found, and the room may be lit up with their rainbow hues for two or three hours, till they are strong enough to brave the open air. It is sad to see the disappointing result if an ichneumon fly has pierced the caterpillar previous to its capture, and laid its murderous eggs inside. Instead of the dazzling, graceful wing of the butterfly, comes out a swarm of dingy little flies, horrible to behold, in their victim's stead. Therefore let these caterpillars be early taken and kept well covered.

We take next a very interesting point, which we believe has not been much noticed. It is the powers of a magnifying glass seen in the eyes of some butterflies. Their eyes may be divided into two classes—*hairy* and *smooth*. The general appearance of the eye is shown at fig. M, where 1, 2, 3 represent the surface lines, as seen with successively increasing powers of the microscope. It is seen that ultimately the surface is found to be covered with a number of hexagonal little lenses, each being, in fact, a complete little eye. About sixty rows of these cover an ordinary eye, such as that of the Vanessa tribe. They are about $\frac{1}{600}$th of an inch at the largest.

Some butterflies have, at the angles joining these hexagonal facets, a hair standing out, which, repeated at thousands of such angles, gives to the eye a rough or hairy look. Such hairy eyes are represented at No. 2. They are very dark. It is in these eyes difficult to see anything but the clothing of hairs.

But other butterflies, such as the white ones, have no hairs in the eye. The eye is then quite smooth and transparent, as at No. 1. Holding such a butterfly closely, with its head turned sideways to the sunlight, a very lovely picture is seen displayed, of an arrangement such as is figured at L, of dark hexagonal spots divided by a network of light-coloured lines. This is brilliantly lit up, and is exactly in appearance like the effect produced by those glass paper-weights which have a picture inside them, magnified by the round surface of the enclosing glass.

The most convenient fly to see this in is the small cabbage one, for it is more easily caught than the larger white. This the writer found from having told his little boy to catch some white butterflies in the kitchen-garden. He always brought in the smaller white, saying that the others were too quick for him. On seeking a reason for this slowness of sight, it is found that the smaller white has the upper part of the eyeball clouded with an opaque shading, as at P Q, which prevents it seeing anyone approach just above it, though leaving the sight clear at the sides.

Taking this conveniently blind little fellow, then, as the subject of examination, easily renewed, the theory of explanation of this appearance evidently is this:—

The hexagonal network on the surface of the eye, as at No. 1, is reflected down on the interior retina in its very small natural size. Then the whole outer surface of the eye, considered as one surface, acts like a globular lens, magnifying the interior reflection till it is easily discerned with the observer's unaided eye, and assumes the very beautiful, picture-like appearance which lasts while the eyes are fresh and moist. Thus the butterfly's eye is a natural magnifying glass, exactly like what is known as a Stanhope lens. The observer uses it as at U, and sees the cells much enlarged.

That the hexagonal appearance is not exactly the same in outline as the actual network on the cornea of the eye is due to the fact of the retina upon which the reflection is cast not being exactly in the focus of the curved lens surface. This leads to an overlapping of the outlines of the facets, making them seem much thicker than the dividing lines between the facets of the surface really are, as at fig. O.

The proboscis or trunk of the butterfly next claims attention.

It is a tube of double structure, each half being easily separated or closed at pleasure by the fly; when not used it is nicely coiled up under the chin. When needed for sucking the juices of plants it is uncoiled, and, the two half tubes being kept together by neat little teeth interlocking, the tube draws up the juice into the mouth of the fly. To enable it to do this, each half of the tube is furnished with a multitude of fibres or nerves, which are reticulated across from side to side, making it easily compressible. The air is exhausted in the tube by this compression, and its place is at once filled by the juice which rises in the tube. Figs. R and S show the general appearance of the tube, fig. T the reticulated fibres for exhausting the air. Near the point of the trunk, an additional means of suction is probably furnished by a number of little protuberances, like nipples, which no doubt help to draw up, like a sponge, by capillary attraction, the juice when only in small quantities. These, however, do not seem to be perforated with suction holes: even when photographed with the microscope on a large scale, they show no sign of any channel through them.

This trunk is, in use, thrust into the nectary of flowers, and in its passage is often covered with pollen. Hence, those butterflies which flit from flower to flower indiscriminately, such as the Peacock or Admiral, would be likely to carry from one flower to another pollen grains, of a useless or different kind, which might interfere with the proper fructification of the flowers they are brought to. Now here comes in one of those wise adaptations of the insect to the vegetable world, which proves the superintendence of one Master Mind. Such butterflies are furnished with a long pair of soft brushes, instead of their forelegs; with which, while using the two other pair of legs to support themselves, they wipe and clean their trunks after each time of using. So that all adhering pollen is wiped off, and not carried to other plants where it is not needed.

In other insects, such as the bee, the provision for preventing pollen mixing is accomplished by a beautiful instinct, which leads the bee to keep only to one class of flower in each trip it takes for honey.

But the butterfly having no settled home, spends its little life in roaming anywhere; and hence has this modified change of legs into brushes, as seen at fig. B.

Its roaming seldom lasts much longer than necessary to meet some mate, and deposit the eggs, like carved ivory balls, as at fig. C, on the plant which will be best fitted for nourishing its young when hatched.

The life of one that has not yet met its mate may be pro-

longed artificially for weeks. Nature's great object being the continuation of the species, life is prolonged in the hope of so doing. Then if allowed its liberty it still seeks its mate ; and perhaps this accounts for the hibernation of such specimens as are the earliest every year ; creeping out with faded glories, to try if they can accomplish their destiny, which was not fulfilled before the winter.

Having settled their eggs on the chosen plant, the parent butterfly never sees them come to maturity : a fair lesson for faith to rejoice in. • Trust may never be felt by the insect, but is evidently taught us through it. And how wonderful is the destined change of the eggs it trusts, while itself will not be there to watch! At all times the glorious change from a first stage of grub or caterpillar, through the trance-like sleep of the chrysalis, into the joyous creature that might well be called a "flutter-by," has been a theme for poets and philosophers.

# ON SLEEP.

By Dr. RICHARDSON, F.R.S.

## The Phenomenon of Sleep.

"THE twinkling of oblivion," as Wordsworth exquisitely defines the phenomenon of sleep, has, from the time of Hippocrates to the present hour, engaged the attention of thoughtful minds. Poets have found in the phenomenon subject matter for some of the most perfect of their works. Menander exalts sleep as the remedy for every disease that admits of cure; Shakespeare defines it, "The birth of each day's life, sore labour's bath;" Sir Philip Sydney designates it, "The poor man's wealth, the prisoner's release;" and wearied Dryden sings of it—

"Of all the powers the best.
Oh! peace of mind, repairer of decay,
Whose balms renew the limbs to labours of the day."

As to the philosophers and the physicians who have said and written on sleep, I dare hardly think of them, lest I should commit myself to an historical volume instead of a short physiological essay; so I leave them, except such as are simply physiological, and proceed on my way.

Perfect sleep is the possession, as a rule, of childhood only. The healthy child, worn out with its day of active life, suddenly sinks to rest, sleeps its ten or twelve hours, and wakes, believing, feeling, that it has merely closed its eyes and opened them again; so deep is its twinkle of oblivion. The sleep in this case is the nearest of approaches to actual death, and at the same time presents a natural paradox, for it is the evidence of strongest life.

During this condition of perfect sleep, what are the physiological conditions of the sleeper? Firstly, all the senses are shut up, yet are they so lightly sealed that the communication of motion by sound, by mechanical vibration, by communication of painful impression, is sufficient to unseal the senses, to arouse the body, to renew all the proofs of existing active life. Secondly, during this period of natural sleep the most impor-

tant changes of nutrition are in progress; the body is renovating, and if young is actually growing; if the body be properly covered, the animal heat is being conserved and laid up for expenditure during the waking hours that are to follow; the respiration is reduced, the inspirations being lessened in the proportion of six to seven as compared with the number made when the body is awake; the action of the heart is reduced; the voluntary muscles, relieved of all fatigue and with the extensors more relaxed than the flexors, are undergoing repair of structure and recruiting their excitability; and the voluntary nervous system, dead for the time to the external vibration, or as the older men called it "stimulus" from without, is also undergoing rest and repair, so that when it comes again into work it may receive better the impressions it may have to gather up, and influence more effectively the muscles it may be called upon to animate, direct, control.

Thirdly, although in the organism during sleep there is suspension of muscular and nervous power, there is not universal suspension; a narrow, but at the same time safe, line of distinction separates the sleep of life from the sleep of death. The heart is a muscle, but it does not sleep, and the lungs are worked by muscles, and these do not sleep; and the viscera which triturate and digest food are moved by muscles, and these do not sleep; and the glands have an arrangement for the constant separation of fluids, and the glands do not sleep; and all these parts have certain nerves which do not sleep. These all rest, but they do not cease their functions. Why is it so?

The reason is that the body is divided into two systems as regards motion. For every act of the body we have a system of organs under the influence of the will, the voluntary, and another system independent of the will, the involuntary. The muscles which propel the body and are concerned in all acts we essay to perform, are voluntary; the muscles, such as the heart and the stomach, which we cannot control, are involuntary. Added to these are muscles which, though commonly acting involuntarily, are capable of being moved by the will: the muscles which move the lungs are of this order, for we can if we wish suspend their action for a short time or quicken it; these muscles we call semi-voluntary. In sleep, then, the voluntary muscles sleep, and the nervous organs which stimulate the voluntary muscles sleep; but the involuntary and the semi-voluntary muscles and their nerves merely rest: they do not veritably sleep.

This arrangement will be seen, at once, to be a necessity, for upon the involuntary acts the body relies for the continuance of life. In disease the voluntary muscles may be paralysed, the

brain may be paralysed, but if the involuntary organs retain their power, the animal is not dead. Sir Astley Cooper had under his care a man who had received an injury of the skull causing compression of the brain, and the man lay for weeks in a state of persistent unconsciousness and repose; practically he slept. He did not die, because the involuntary system remained true to its duty; and when the great surgeon removed the compression from the brain of the man, the sleeper woke from his long trance and recovered. Dr. Wilson Philip had a young dog that had no brain, and the animal lay in profound insensibility for months, practically asleep; but the involuntary parts continued uninfluenced, and the animal lived and, under mechanical feeding, grew fat. Fluorens had a brainless fowl that lived in the same condition. It neither saw nor heard, he says, nor smelled nor tasted nor felt; it lost even its instincts; for however long it was left to fast, it never voluntarily ate; it never shrunk when it was touched, and when attacked by its fellows, it made no attempt at self-defence, neither resisting nor escaping. In fine, it lost every trace of intelligence, for it neither willed, remembered, felt, nor judged: yet it swallowed food when the food was put into its mouth, and fattened. In these cases, as in that of the injured man, the involuntary systems sustained the animal life. It is the same in sleep.

When we look at these phenomena, as anatomists, we find a reason for them in structure and character of parts. The involuntary muscles have a special anatomical structure; and the nervous organism that keeps the involuntary muscles in action is a distinct organism. There are, briefly, two nervous systems: one locked up in the bony cavity of the skull and in the bony canal of the spine, with nerves issuing therefrom to the muscles; and another lying within the cavities of the body, with nerves issuing from it to supply all the involuntary muscles. The first of these systems, consisting of the brain, the spinal cord, and the nerves of sense, sensation, and motion, is called the cerebro-spinal or voluntary system of nerves; the second, consisting of a series of nervous ganglia with nerves which communicate with the involuntary muscles and with nerves of the voluntary kind, is called, after Harvey the vegetative, after Bichat the organic system: a sketch of this organic system is depicted in the accompanying diagram.

In sleep the cerebro-spinal system sleeps; the organic system retains its activity. Thus in sleep the voluntary muscles and parts fail to receive their nervous stimulation; but the involuntary receive theirs still, and under it move in steady motion; while the semi-voluntary organs also receive sufficient stimulation to keep them in motion.

Of all the involuntary organs, the heart, which is the citadel of motion, is most protected. To itself belongs a special nervous centre, that which feeds it steadily with stimulus for motion; from the cervical ganglia of the organic nervous system it receives a second or supplementary supply; and from the brain it receives a third supply, which, passive under ordinary circumstances, can under extraordinary circumstances become active and exert a certain controlling power. Then the arteries which supply the heart with blood are the first vessels given off from the great feeding arterial trunk, and the veins of the heart winding independently round it empty their contents direct again into it. Thus is the heart the most perfect of independencies: thus during sleep and during wakefulness it works its own course, and taking first care of itself in every particular, feeds the rest of the body afterwards; thus even when sleep passes into death the heart in almost every case continues its action for some time after all the other parts of the organism are in absolute quiescence; thus in hybernating animals the heart continues in play during their long somnolence; and thus under the insensibility produced by the inhalation of narcotic gases and vapours, the heart sustains its function when every other part is temporarily dead. Next the heart in independent action is the muscle called the midriff or diaphragm; and as the diaphragm is a muscle of inspiration, the respiratory function plays second to the circulatory, and the two great functions of life are, in sleep, faithfully performed. In sleep of illness bordering on sleep of death, how intently we watch for the merest trace of breath, and augur that if but a feather be moved by it or a mirror dimmed by it, there is yet life.

In natural sleep then, sleep perfect and deep, that half of our nature which is volitional is in the condition of inertia. To

say, as Blumenbach has said, that in this state all intercourse between mind and body is suspended, is more perhaps than should be said, the precise limits and connections of mind and body being unknown. But certainly the brain and spinal cord, ceasing themselves to receive impressions, cease to communicate to the muscles they supply stimulus for motion, and the muscles under their control with their nerves therefore sleep. And so, to the extent that the acts of the brain and cord and their nerves are mental, and the acts or motions of the voluntary muscles are bodily acts; to that extent, in sleep, the intercourse between the mind and the body is suspended.

### The Physical Cause of Sleep.

In sleep the condition of the voluntary muscles and of the voluntary nervous system is, we must assume, in some manner modified, since these organs are transformed from the active into the passive state. Respecting the condition of the muscles in sleep, no study of a systematic sort has been carried out, but in relation to the brain there has been much thoughtful study, upon which many theories have been founded.

The older physiologists regarded sleep as due to the exhaustion of the nervous fluid; during sleep, they held, this fluid accumulates in the brain; and, when the brain and the other centres and nerves of the cerebro-spinal system are, to employ a common expression, recharged, the muscles are stimulated and the body awakes; the brain prepared to receive external impressions and to animate the muscles, and the muscles renovated and ready to be recalled into activity. This theory held its ground for many years, and, perhaps, still there are more believers in it than in any other. It fails to convince the sceptical because of its incompleteness, for it tells nothing about the nature of the presumed nervous fluid, and we know nothing as yet about this fluid. The primary step of the speculation is consequently itself purely hypothetical.

Another theory, that has been promulgated, is that sleep depends on the sinking or collapse of the laminæ of the cerebellum or little brain. This theory is based on the experiment that compression of the cerebellum induces sleep; but the argument is fallacious, because pressure on the larger brain, or cerebrum, is followed by the same result. The theory of pressure has been proposed again in a different way; it has been affirmed that the phenomenon of sleep is caused by the accumulation of fluids in the cavity of the cranium, and by pressure, resulting from this accumulation, on the brain as a whole. We know well that pressure upon the brain does lead to an insensible condition resembling sleep, and in some instances, in

which the skull has been injured and an artificial opening through it to the brain has been formed, pressure upon the exposed surface has led to a comatose condition. I once myself saw a case of this nature. But the evidence against this explanation is strong, because the sleeping brain has been observed to be pale and too free of blood to convey any idea of pressure.

In opposition to the pressure theory, Blumenbach contended that sleep is due to a diminished flow and impulse of blood upon the brain, for he argued the phenomenon of sleep is induced by exhaustion, and particularly by exhaustion following upon direct loss of blood. Recently Mr. Arthur Durham, in a very able communication, has adduced a similar view, and the general conclusion now is that during sleep the brain is really supplied with less blood than in waking hours.

To account for the reason why the brain is less freely fed with blood in sleep, it has been surmised that the vessels, the arteries, which feed the brain, and which for contractile purposes are supplied with nerves from the organic nervous system, are, under their nervous influence, made to close so that a portion at least of the blood which enters through them is cut off on going to sleep. This view, however, presupposes that the organic nervous centres, instead of sharing in the exhaustion incident to labour, put forth increased power after fatigue, an idea incompatible with all we know of the natural functions.

Carmichael, an excellent physiologist, thought that sleep was brought on by a change in the assimilation of the brain, and by what he called the deposition of new matter in the organ, but he offered no evidence in proof: while Metcalfe, one of the most learned physicists and physicians of our time, maintained that the proximate cause of sleep is an expenditure of the substance and vital energy of the brain, nerves, and voluntary muscles, beyond what they receive when awake, and that the specific office of sleep is the restoration of what has been wasted by exercise; the most remarkable difference between exercise and sleep being, that during exercise the expenditure exceeds the income; whereas during sleep the income exceeds the expenditure. This idea of Metcalfe expresses, probably, a broad truth, but it is too general to indicate the proximate cause of sleep, to explain which is the object of his proposition.

My own researches on the proximate cause of sleep—researches which of late years have been steadily pursued—lead me to the conclusion that none of the theories as yet offered account correctly for the natural phenomenon of sleep; although I must express that some of them are based on well-defined facts. It is perfectly true that exhaustion of the brain will induce phenomena so closely allied to the phenomena of natural sleep, that no one could tell the artificially induced from

the natural sleep; and it is equally true that pressure upon the brain will also lead to a state of sleep simulating the natural. For example, in a young animal, a pigeon, I can induce the deepest sleep by exposing the brain to the influence of extreme cold. I have had a bird sleeping calmly for ten hours under the local influence of cold. During this time the state of the brain is one of extreme bloodlessness, and when the cold is cautiously withdrawn and the brain is allowed to refill gently with blood, the sleep passes away. This is clear enough, and the cold, it may be urged, produces contraction of the brain substance and of the vessels, with diminution of blood, and with sleep as the result. But if when the animal is awaking from this sleep induced by cold, I apply warmth, for the unsealing of the parts, a little too freely, if, that is to say, I restore the natural warmth too quickly, then the animal falls asleep again under an opposite condition; for now into the relaxed vessels of the brain the heart injects blood so freely, that the vessels, in like manner as when the frozen hand is held near the fire, become engorged with blood, there is congestion, there is pressure, and there is sleep.

The same series of phenomena from opposite conditions can be induced by narcotic vapours. There is a fluid called chloride of aonyl, which, by inhalation, causes the deepest sleep; during the sleep so induced, the brain is as bloodless as if it were frozen. There is an ether called methylic, which, by inhalation, can be made to produce the deepest sleep; during this sleep the vessels of the brain are engorged with blood.

We are therefore correct in supposing that artificial sleep may be induced both by removal of blood from the brain, and by pressure of blood upon the brain; and in the facts there is, when we consider them, nothing extraordinary. In both conditions, the natural state of the brain is altered; it cannot, under either state, properly receive or transmit motion; so it is quiescent, it sleeps. The experimental proof of this can be performed on any part of the body where there is nerve-fibre and blood-vessel; if I freeze a portion of my skin, by ether spray, I make it insensible to all impression—I make it sleep; if I place over a portion of skin a cupping tube, and forcibly induce intense congestion of vessels, by exhausting the air of the tube, I make the part also insensible—I make it sleep.

The two most plausible theories of sleep—the plenum and the vacuum theories I had nearly called them—are then based on facts; but still I think them fallacious. The theory that natural sleep depends on pressure of the brain from blood, is disproved by the observations that have been made of the brain during sleep, while the mechanism of the circulation through the brain furnishes no thought of this theory as being possibly

correct. The theory that sleep is caused by withdrawal of blood from the brain by contraction of its arterial vessels, is disproved by many considerations. It presupposes that at the time when the cerebro-spinal nervous system is most wearied the organic system is most active; and it assumes that the great volume of blood which circulates through the brain can be cut off without evidence of increased volume of blood and tension of vessel in other parts of the body, a supposition directly negatived by the actual experiment of cutting off the blood from the brain.

There is another potent objection applicable to both theories. When sleep is artificially induced, either by subjecting the brain to pressure of blood or to exhaustion of blood, the sleep is of such a kind that the sleeper cannot be roused until the influence at work to produce the sleep is removed. But in natural sleep the sleeper can always be roused by motion or vibration. We call to a person supposed to be sleeping naturally, or we shake him, and if we cannot rouse him we know there is danger; but how could these simple acts remove pressure from the brain, or relax the contracted vessels feeding the brain?

These two theories set aside, the others I have named need not trouble us; they are mere generalisations, interesting to read, worthless to pursue. Know we then nothing leading towards a solution of the question of the proximate cause of sleep? I cannot say that, for I think we see our way to something which will unravel the phenomenon; but we must work slowly and patiently, and as men assured that in the problem we are endeavouring to solve, we are dealing with a subject of more than ordinary importance. I will try to point out the direction of research.

I find that to induce sleep it is not necessary to produce extreme changes of brain matter. In applying cold, for example, it is not necessary to make the brain substance solid in order to induce stupor, but simply to bring down its temperature ten or twelve degrees. I find also that very slight direct vibrations, concussions, will induce stupor; and I find that in animals of different kinds the profoundness of sleep is greater in proportion as the size of the brain is larger. From these and other facts I infer that the phenomenon of natural sleep is due to a molecular change in the nervous structure itself of the cerebro-spinal system, and that in *perfect* sleep the whole of the nervous structure is involved in the change—the brain, the cord, the nerves; while in imperfect sleep only parts of this nervous matter are influenced. This is in accord with facts, for I can by cold put to sleep special parts of the nervous mass without putting other parts to sleep. In bad sleep we

have the representation of the same thing in the restlessness of the muscles, the half-conscious wakings, the dreams.

Suppose this idea of the change of nervous matter to be true, is there any clue to the nature of the change itself? I think there is. The change is one very closely resembling that which occurs in the solidification of water surcharged with a saline substance, or in water holding a hydrated colloid, like dialysed silica, in trembling suspension. What is, indeed, the brain and nervous matter? It is a mass of water made sufficiently solid to be reduced into shape and form, by rather less than twenty per cent. of solid matter, consisting of albuminous substance, saline substance, fatty substance. The mechanism for the supply of blood is most delicate, membranous; the mechanism for dialysis or separation of crystalloidal from colloidal substance is perfect, and the conversion of the compound substance of brain from one condition of matter to another is, if we may judge from some changes of water charged with colloidal or fatty substances, extremely simple. I do not now venture on details respecting this peculiarly interesting question, but I venture so far as to express what I feel will one day be the accepted fact, that the matter of the wakeful brain is, on going to sleep, changed, temporarily, into a state of greater solidity; that its molecular parts cease to be moved by external ordinary influences, by chemical influences; that they, in turn, cease to communicate impressions, or, in other words, to stimulate the voluntary muscles; and that then there is sleep which lasts until there is re-solution of structure, whereupon there is wakefulness from renewed motion in brain matter and renewed stimulation of voluntary muscle, through nerve.

The change of structure of the brain which I assume to be the proximate cause of sleep is possibly the same change as occurs in a more extreme degree when the brain and its subordinate parts actually die. The effects of a concussion of the brain from a blow, the effects of a simple puncture of nervous matter in centres essential to life—as the point in the medulla oblongata which Fluorens has designated the vital point—have never been explained, and admit, I imagine, of no explanation except the change of structure I have now ventured to suggest.

Here, for the moment, my task must end. My object has been to make the scientific reader conversant with what has been said by philosophers upon the subject of sleep and its proximate cause, and to indicate briefly a new line of scientific enquiry. I shall hope on some future occasion to be able to announce further and more fruitful labour.

# REVIEWS.

## THE TRANSFORMATION OF INSECTS.*

AMID the vast host of drawing-room scientific works which the past few years have introduced into England, the best, it seems to us, is the work of M. Blanchard, edited by Dr. Duncan, which is now before us. Whether we consider the marvellous array of exquisitely beautiful plates, a host of woodcuts, the admirable printing, or last, but not the least, the excellent additions which our English naturalist has made to the text, we must readily give the first place to M. Blanchard's treatise.

Curiously enough, it deals not only with the animals of one class alone, but, besides treating of the insects, it goes comparatively fully into the *Arachnida* and *Crustacea*. In this respect it presents, apart from the many features of novelty which we find in other parts of the volume, much that is new to the general reader. For it must be confessed that, while the insecta are comparatively well known in many respects to the world generally, the *Arachnids* and *Crustaceans* form—especially the latter—rather new groups for the consideration of the naturalist.

The first chapters, of course, deal with questions of physiology, such as those of metamorphosis, and the general character of the animals described, and the succeeding ones with the metamorphoses of the various groups of insects; then in the same manner, though in briefer fashion, are subsequently detailed the classes *Myriapoda*, *Arachnida*, and *Crustacea*. In the earlier chapters—those, in fact, which deal with the general anatomy and physiology of each group—we fancy that Dr. Duncan has had the heaviest work to perform. Not merely as a translator, of course, but in bringing the French edition up to the state of our recent knowledge. We have only seen the French book momentarily, and of course we may be wrong; but we fancy that a great deal of new matter meets the eye in this part of the work, and we heartily thank Dr. Duncan for the trouble he has taken; for, if we do not

* "The Transformation, or Metamorphoses of Insects" (*Insecta Myriapoda, Arachnida, Crustacea*). Being an Adaptation, for English Readers, of M. Émile Blanchard's "Métamorphoses, mœurs et instincts des Insectes," and a Compilation from the Works of Newport, Charles Darwin, Spence Bate, Fritz Müller, Packard, Lubbock, Stainton, and others. By P. Martin Duncan, F.R.S., Professor of Geology in King's College, London. London; Cassell, Petter, & Co. 1871.

mistake, he has been at pains to make a work, that was intended merely as a popular one, do service of a good kind to both naturalists and the public generally. We believe that it is he who introduced much that relates to the nervous system of insects, for we find in this work a great deal of matter that is absolutely new to popular treatises. Especially so is the account of the development of the nervous system in the larva of *Vanessa urticæ*, which is taken from that wondrous worker among insects, our English Newport, and which is briefly as follows:—"Two hours after the larva of *Vanessa urticæ* has suspended itself to undergo its transformation, and in which state it remains from six, eight, ten, or even twenty-four hours—according to the strength of the individual, and other circumstances—before it throws off its last larva-skin, a considerable alteration has already taken place in the body of the larva. The ganglions in the head are still distinct from each other, but are a little in form, although not yet enlarged. The sub-œsophageal ganglion is enlarged to nearly twice its original size, and the cords which join it to the brain are shortened, and so are those that connect the second, third, fourth, and fifth ganglions. The last two are separated only by a short interval. The fifth, sixth, and seventh are drawn closer together, the ends between them are disposed in a zig-zag manner, and the longitudinal direction of the nervous chain is in consequence altered. The ganglions, from the seventh to the terminal one, remain as in the active larva. A little while before the old skin is thrown off there is great excitement throughout the body of the insect. About half-an-hour before this occurs there is a considerable enlargement of the brain, the sub-œsophageal, and the second, third, fourth, and fifth ganglions. The cords that extend between them diverge very much, and those between the fifth, sixth, and seventh are disposed in a zigzag direction. Immediately after the insect hasentered the pupa state all the ganglions are brought closer together, in consequence of the ends being disposed more irregularly than at any other period, which has been occasioned by the shortening that has taken place in every segment, by which the cords are rendered too long to lie in a direct line. Seven hours afterwards there is a greater enlargement of the brain, optic nerves, and ganglions of the thoracic segments, which begin to approach each other. At twelve hours the thoracic ganglions have united, and at eighteen the nerves of the wings have increased in size, and the nervous chain in the abdomen has become straight again. At thirty-six hours the optic nerves have grown nearly as large as the brain, and the gullet is completely surrounded by an extension of the ganglion under it, and the brain above. The sixth ganglion has disappeared; and at the end of forty-eight hours the seventh is no longer seen, but the thoracic centres increase in size gradually. At fifty-eight hours the middle part of the chest has greatly increased in size, and the great nervous centres and nerve twigs, which will supply the wings eventually with energy, occupy it. The optic and antennal nerves have nearly attained their full development, and the arrangement of the whole nervous system is now nearly as it exists in the perfect insect. The whole of these important changes are thus seen to take place within the first three days after the insect has undergone its metamorphoses, and they precede those of the digestive system." We have given this rather long quotation for various reasons: in the first place, it illustrates

a wonderful phase in normal development; again, it shows the marvellous insight of Newport, who must have spent many and many a weary hour ere he could lay down tersely the information above given. Lastly, it shows how carefully the editor (for we credit him with the quotation) has brought the work to its proper development in thus giving the general reader some small idea of the amount of work done by a labourer who is known but to anatomists, but who deserves to be remembered when many of the present race of naturalists are no more.

But the general reader, uninterested in metamorphoses, may think the above of very little significance, and may naturally be more interested in the general habits or other conditions of insects. If he be, he cannot fail to find abundance to interest him in this work. For example, let us take some of the curious facts relating to parthenogenesis, which are recorded, and for which we think again we are indebted to Dr. Duncan. Von Siebold, he says, "collected a great number of the cases, or sacs, as he calls them, of *Solenobia lichenella and S. triquetrella*, and to his great astonishment none but female individuals came out of them, and only a single locality furnished him with a couple of males. He kept these females carefully in little vessels closed with glass lids, and found that they clung to their cases, resting upon the outside of them. These virgin females laid eggs and filled their sacs with them, and did not wait for a fertilising male, for they commenced egg-laying very soon after they escaped from the pupa-case or the chrysalis condition. When the *Solenobiæ* were removed from their sacs, they had such a violent impulse to lay, that they pushed their laying tube about in search of the surface of the sac, and at last let their eggs fall openly. He writes, 'If I had wondered at the zeal for oviposition in these husbandless *Solenobiæ*, how was I astonished when all the eggs of these females, of whose virgin state I was most positively convinced, gave birth to young caterpillars which looked about with the greatest assiduity, in search of materials for the manufacture of little sacs.'" This is curious enough, but is just a mite from the immense store of information which the book contains; and we only wish that our space permitted us a more extensive quotation. But we have said, we hope, sufficient to show generally the great value and importance of the work, which is an admirable, tersely written, English work, and whose engravings are beyond comparison as they are almost beyond number.

## ELEMENTS OF MECHANISM.*

THE present work is one of a series which Messrs. Longmans are issuing, as we suppose, in opposition to the many works of a somewhat similar kind which are being circulated about at present. It is one of a series which the editor tells us is intended for the members of a large class which

* "The Elements of Mechanism." Designed for Students of Applied Mechanics. By T. M. Goodeve, R.A., Lecturer on Applied Mechanics at the Royal School of Mines. London: Longmans. 1870.

has only become students of late years. Will it serve its purpose? We cannot say in exact terms, but we can tell what our impression is, and that is to the effect that it will *not*. In no way is this due to Messrs. Longmans, who have displayed as much care in the publishing of this work as of any other, and who have turned it out in such a manner that, for illustration and typography, it is inferior to none; but we fancy that the editor and the author have both gone upon the wrong scent. It is true that against this view is the fact that the work has gone through two editions already; yet must we express the opinion, that it is not exactly the work suited to the student of mechanics. The book is of course addressed to a different class of men from those for whom most similar writings are intended. In fact it is not intended for the ordinary student at all. Yet may we not say that the engineering and mechanical pupil, *pure and simple*, will not understand it if perchance it be the first work of the kind placed in his hands. For ourselves, we confess that if the work now before us—ably and intelligibly written as it is—were the first book given us as a manual of mechanics, we should fail to follow it in such a manner as it should be followed by the student. Yet may it prove quite the opposite with most students, and we heartily hope it may, for, though quite different from most treatises on mechanics, it is admirable in all its parts, and is most clearly and intelligibly written by the author. It is the first of Messrs. Longmans' series that we have seen, and we have judged rather unfavourably of its appearance.

## A MANUAL OF COLOURS.*

A CURIOUS book, and one which not often presents itself to the reviewer. What shall we call it, or how shall we describe it? It is not strictly, as its name implies, a manual of colours, for it contains many other subjects; but it is mainly such information as those interested in the chemistry of colours require. It is, in fact, an elementary manual of dyeing, and yet it is not entirely so. But if the reader will imagine a book in which is contained all the information that a scientific dyer requires—that is, a dyer who is prepared to go into the chemistry of dyes—he will form a good idea of the nature of the work. It is got up in dictionary form, which we think most excellently convenient for such a book—each paragraph being commenced by a titular word in deep black letters, or Egyptian type. What shall we say of the work? We cannot afford it much praise, for the reason that it by no means equals the subject it proposes to discuss. But then comes the question, Can you have better? and to this we fear must be answered, No. The fact is that, save a rudimentary manual, or one considerably behind the time, none can now be written. Why? the uninitiated will inquire. For the simple and very intelligible reason that if it dealt with the subject in such a manner that all should be able to glean from it the different methods of dyeing, each would

* "The Manual of Colours and Dye-Wares, their Properties, Applications, Valuation, Impurities, and Sophistications, &c." By J. W. Slater. London: Lockwood & Co. 1870.

be able to adopt the process, and so while he might benefit the State he would certainly ruin his neighbours. But his neighbours are too cunning for this; so when they discover a secret in dyeing, or any other chemical process out of which money can be turned, they simply keep it to themselves, and make money out of it while they can. The author, therefore, cannot get hold of it, and of course he cannot put it into his book. But, with this exception, Mr. Slater's manual is a capital little work, well arranged, and with almost sufficient information on all subjects connected with the dyer. The reader, taking these facts into consideration, will, we should think, consider the manual a convenient and well-informed assistant, as we do.

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## TERSE GEOLOGY.*

HERE is a curious book; a small one, and nevertheless a very useful one, which we wonder, now that we see it, has not made its appearance earlier. It is evidently a concise, clear, and well-arranged note-book to the lectures of the eminent men who have produced it. We say a note-book, for of the materials of a note-book does it consist; but it must be remembered that it is the note-book not of a student, but of a professor, and one too which emanates from two men, one of whom is one of the most thorough geologists in the kingdom. It is intended as a guide to the courses of lectures of two professors of geology and mineralogy—a very full note-book, to be sure, but still merely a note-book, to assist the student in placing together a vast amount of information to be derived from other sources. This is what the authors say of it:—"The student will, on his part, find clear statements and explanations of the things, facts, and circumstances on which geology is based, whether he reads lecture by lecture, or begins with some particular subject, or according to some chosen classification, such as either of the eight courses of lectures may supply. In either case he must make the matter his own by cross-references, using the index as explained in its prefatory note. Thus he will pursue a systematic study of every subject and set of subjects, either according to the lectures, one of the Synopses, or any plan that his own reading or his teacher may suggest. Only by *classifying* on some principle or other can a student master scientific knowledge." We can only say that if the student follows the advice above given, he will become master of geology in a comparatively short period, and he will have learned one of the best methods of making himself familiar with a complex subject. We have only one regret, and that is the want of a few typical illustrations. These are certainly required; and we trust that in the next edition, which must be nearly ready even now, the authors will take our advice so far. In every other particular the work, though small, is everything we can desire.

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* "Geology." By John Morris, F.G.S., Professor of Geology and Mineralogy in University College, London; and T. Rupert Jones, F.G.S., Professor of Geology and Mineralogy, Royal Military College, Sandhurst. First Series. Van Voorst. 1870.

## NATURAL PHILOSOPHY.*

THE book now before us is an English rendering of the French work published a few years since in Paris, and written by M. A. P. Deschaud. Professor Everett, D.C.L., the teacher of Natural Philosophy in Queen's College, Belfast, has rendered it in English, and it has now appeared in its first part—that which is devoted to mechanics, hydrostatics, and pneumatics. We should have thought that there were sufficient works on this subject in England without the introduction of a French volume. We could name at least half-a-dozen which we think we could prove to be quite as excellent as M. Deschaud's treatise; but then that is nothing; a very slight difference is sufficient, to the mind of a professor in a new chair, who desires to make himself known to the world, to induce him to introduce a new work. We suppose that really the amount of difference between the present work and any of the various treatises which exist already, is, to the reader, extremely small, however great it may seem to the translator. But the translator is the person responsible, and he introduces the work. However, we may merely call attention to the book as a very good one, and we suppose our thanks are due to the gentleman who has introduced it into England. His own work has not been extremely heavy, but, as far as it has gone, it is good, though rendered here and there in a less simple style than we think might have been adopted. The book has many excellent illustrations, and is remarkably well printed. Why it has been issued in a flexible cover we fail to see, though doubtless there are good reasons in the publisher's mind.

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## THE TRUTH OF THE BIBLE.†

AGAIN! A work on Biblical truth. Year after year some one comes forward to support the Bible against men of science, and we humbly ask the reason why? Do scientific men engage their attention in writing against Biblical records? Do Lyell and Huxley, or Murchison and Hooker, take the trouble to write huge books against the Bible, that it finds so many to support it? Or is it that Church of England clergymen, with little of scientific knowledge, and with a courage proportionate to their ignorance, are anxious to make what they can by their penmanship? We fear this has something to do with it; for we do not, we think, find that those who are conversant with the study of science are, as a rule, the most ready to rush into the field, prepared to hold a particular view of the Bible. No; on the contrary. It is some country curate, gifted with abundance of time, learned in a few books, and worshipped by a number of old ladies, who lays

* "Elementary Treatise on Natural Philosophy." By A. Privat Deschaud. Translated and edited by J. D. Everett, M.A., D.C.L., Professor of Natural Philosophy in Queen's College, Belfast. Part I. London: Blackie. 1870.

† "The Truth of the Bible; Evidence from the Mosaic and other Records of Creation; the Original Antiquity of Man," &c., by the Rev. Bourchier Wrey Savile, M.A., Curate of Coombe. London: Longmans. 1871.

his whole strength to settle down, finally and clearly, that which, during the present century, has been the greatest mystery to all who have entered upon it. Such men are ever ready, at a few months' notice, to take up any subject, and satisfy themselves that they understand it fully. They forget how many have already been in the field—how numerous have been the different views as to Bible and Science reconciliation—and, finally, how instantly their ideas would have been rejected less than one hundred years ago. It would be idle to examine this work, which is merely a sort of scrap-book, containing extracts from the many men who have written upon the subject, without any adequate conception of the matter by the author, who is simply one of those men who are prepared *against* science, and who necessarily come into the world and leave it without influencing in the *slightest degree* the views of those who are capable of forming opinions. He holds the views he states—that is all. He quotes a number of writers to show that scientific men differ much among themselves. He has given a book full of quotations of this kind; in fact, it is what his book consists of. But then, we ask, what on earth does it prove? That men disagree? We admit it, of course; what would the world be if they didn't? But if the reader cannot see that the conclusions of scientific minds are, as years advance, becoming more and more distinct from the ideas of the Old Testament, and that such conclusions are in general terms complete, we pity them. The Old Testament is losing much of its force as it was laid down a hundred years ago, but we do not see that this lessens Christianity in the slightest degree. We deplore books like the present, written by churchmen, in a bitter spirit of hostility to men of science, and impossible to be read by those who are desirous of knowing nothing but the truth, and who seek it to their own cost, and their own disadvantage. Does the author think his book will satisfy a single really scientific sceptic? If so, he is mistaken. And yet, we ask, is it not the fact that every sceptic would desire to be convinced that there was a future life in store for him? Can the Christian conceive that life in this world is so delightful, that man would not hail with the supremest delight the belief in a future and a God, did he really imagine before that they were not? It is impossible for the thoughtful mind to imagine so wilful a sceptic, and it is for that reason that we think that a book which is written in the style that this one displays, in pages 174, 5, 6, is one entirely unsuited to the sceptical mind.

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## A "HANDBOOK OF THE TELEGRAPH."*

WE know of no better book than this to place in the hands of a beginner, and we think, now that the management of the telegraph has been practically placed in the hands of the Government, that it should place this work on the table in every office in the kingdom. It is essentially a book which should be in such a place, for it especially deals with telegraphy as

* "Handbook of the Telegraph." Being a Manual of Telegraphy. By R. Bond. Third edition. Lockwood. 1870.

an art, and we know of no work from which so much may be gathered in so short a time. Its arrangement has been well planned, and its matter is excellently arranged, and, so far as we can see, made intelligible to the feeblest by putting the difficult problems in a multitude of ways. It is so arranged that anyone, be he ever so simple, can understand it. It may seem unusual, but we cannot avoid giving the contents; for they show, even more than any words of ours, how admirably the scheme of the work is carried out. The first part is of course introductory. Then follows advice to the student as to his form of application, various examples of the subject of examination being given; and next we have information as to officials. Then follow in proper order a long list:—scale of charges, delivery of telegrams, double needle instrument, single needle instrument, Morse's printing instrument, grouping of letters of different alphabets, Hughes' printing instrument, bell instrument, Wheatstone's automatic machine, Wheatstone's ABC instrument, pneumatic code, postal telegraph codes, miscellaneous regulations, inland telegraph-forms, abstracts, abstract books, accounts, returns, railway companies' messages and forms, railway-train telegraphs, offences, useful hints, telegrams and index. Altogether a most valuable, indeed invaluable, amount of information to the telegraph clerk.

## CHEMISTRY AND METALLURGY.*

HERE we have two books in Messrs. Longmans' "Text-book of Science" series. They have quite recently reached us, and we are compelled to say of them that they undo the impression left upon our mind by the Manual of Machinery which we had seen as the first, and which we have noticed rather unfavourably. The present two works have left a satisfactory feeling upon our mind. They bear out the view expressed by the publishers, that "the works will not be manuals for immediate application, nor university text-books, in which mental training is the principal object; but are meant to be *practical treatises, sound and exact in their logic, and with everything and every process reduced to the stage of direct and useful application, and illustrated by well-selected examples from familiar processes and facts*. It is hoped that the publication of these books—in addition to other useful results—will tend to the leading up of artisans to become candidates for the Whitworth Scholarships." We think, as we have said, that the authors of the present works—one of them unhappily removed from among us—have done their work well, and in accordance with the plan on which the volumes themselves are laid down.

And first of Mr. Bloxam's labours. His has been the most laborious task. Metallurgy is not a subject so frequently treated of as Chemistry. He had,

* "Metals; their Properties and Treatment." By Charles Loudon Bloxam, Professor of Practical Chemistry in King's College, London. London: Longmans. 1870.

"Introduction to the Study of Inorganic Chemistry." By William Allen Miller, M.D., D.C.L., late Treasurer and Vice-President of the Royal Society, Professor of Chemistry in King's College, &c. Longmans. 1871.

therefore, in bringing it within the compass of a small octavo volume, a somewhat difficult duty, and yet do we think he has done it well. Of course it must not be imagined that he has treated of all the metals. Such a scheme would be beyond his plan, as it would most unquestionably have been beyond his pupils. He has therefore, of course, selected just those metals which are eminently useful, not merely of special interest; and has given us minutely, and yet clearly, the modes, step by step, followed in procuring them from their ores, from the time the crude ore is taken in hand till the metal itself is discharged. He has given us the methods employed in the metallurgy of iron and steel, copper, tin, zinc, lead, silver, gold, mercury, platinum, palladium, antimony, bismuth, aluminium, magnesium, and cadmium. Indeed, some will say that he has dealt in a few instances with metals which have little or no real use in the arts, but we may remark that in these cases he has merely given a page or so each, so that they do not take up much space. But with the metals which come into daily use he has dealt liberally enough. Thus iron and steel come in for quite a third of the whole volume. The description of these is full of interest, especially as the author has introduced a description of the Bessemer and various allied processes. This and the other parts of the volume are amply illustrated, and the illustrations are clear enough, but they strike us, merely from their appearance, as being either badly cut or used for some years, but this may be due to the fact that our copy is an imperfect reprint. Altogether we are very much pleased with the way in which Mr. Bloxam has discharged his task, and we hope the other volumes of the series will compare with his favourably.

The late Dr. Miller's work has been less difficult than the former author's, yet do we think it has been equally well done. We had expected that it would have been little more than a cutting down of the author's well-known large volume (Vol. II.) on Inorganic Chemistry. It seems, however, to be a work written for a special purpose, and to be well adapted to the object the writer had in view. It was much to be regretted that the author should have died before the book came out; but, so far as care, trouble, and energy could have been exerted upon the most laborious part of the writer's duties, that of "seeing it through the press," his friend Mr. C. Tomlinson, F.R.S., has been an able and a thorough friend. There is not much, of course, to say about the volume. It appears to be carefully executed, as we might have expected; but there is one feature which we very much admire, and that is the introduction of experiments, within easy range of the student, and calculated to impress the foregoing matter upon his mind. These are numerous, simple, and cheaply performed, and we are glad of this introduction in such a volume as the present one; altogether it is, like Professor Bloxam's, an excellent little volume, and we heartily wish both every success in the world.

## COFFEE-PLANTING.*

THIS is the second edition of a work which, of course, can have little interest for the great majority of our readers. Yet is it a special work, well done, and of considerable value to a certain number of persons. It is the practical experience of a gentleman who has had no less than twenty years' work in the districts of Pussilava, Hewahette, and Rambodde. It must therefore be good, and, so far as we can judge, it is extremely full, and really is rather generally interesting. The author has not supplied his own experience alone. He has taken numerous extracts from Labories' well-known treatise, which, though old, is still an excellent work. In addition to this, he has given the substance of various letters, and a quantity of matter, from Ferguson's "Ceylon Directory," 1864–5. Altogether he has tried to make his book interesting and useful, and we think he has succeeded.

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## OTHER WORLDS THAN OURS.†

THIS, the second edition of Mr. Proctor's celebrated treatise, reaches us too late to admit of our giving to it more than a casual notice. Still we must say a word or two. The preface strikes us as being not the least interesting portion, for therein does the author deal with those opponents of his ideas who have endeavoured by unfair means to take from him his own ideas, and who, desirous of not recognising his exposure of their errors, have dealt with him in a manner not characterised by that openness and honesty which some scientific men would have at once exhibited. We have read Mr. Proctor's remarks in this part of his work with considerable care; and, so far as we can see, he has (considering the treatment he has received from certain opponents) been not a whit too severe, while at the same time his arguments appear most just, and if Professor Pritchard and Mr. N. Lockyer do not give us some satisfactory reply, we shall of course be compelled, greatly to their disadvantage, to accept to the fullest degree the explanation Mr. Proctor has so clearly given. Among the more important novelties in this edition we may refer to the evidence against the theory that the cloud-belts of Jupiter and Saturn are raised by the sun's heat. He considers that the forces inherent in these planets are abundantly sufficient to account for the phenomena. The author also endeavours to demonstrate that there are laws of stars, aggregative and segregative, other than those laid down, and he believes that the Milky Way is *not* a structure of stars of all orders, but is, in fact, a small stream "amidst which many of the lucid stars are immersed." As regards Mr. Proctor's last chapter, we can offer no opinion whatsoever.

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* "The Coffee Planter of Ceylon." By William Sabonadière. Second edition. London: Spon. 1870.

† "Other Worlds than Ours. The Plurality of Worlds studied under the light of Recent Scientific Researches." By Richard S. Proctor, B.A., F.R.A.S. Second edition. London: Longmans. 1870.

## METHOD AND MEDICINE.*

DR. FOSTER has in this volume dealt with the mode in which the medical man should await knowledge—not content with what is known, not over desirous of adding novelties to science, but between the two. His advice is excellent, and is what we should expect from one who has had such ample experience both as physician and teacher. The volume is an essay which originally appeared among a series of papers by the members of the Birmingham Speculative Club, from which the author has, in our opinion, done well to reproduce it. It displays at once the scholarship of an able physician and the style of a by no means inexperienced writer.

* "Method and Medicine." An Essay. By Balthazar W. Foster, M.D., Professor of Medicine in Queen's College, and Physician to the General Hospital, Birmingham. London: Churchill. 1870.

# SCIENTIFIC SUMMARY.

## ASTRONOMY.

THE *Eclipse Expeditions.*—The arrangements made by the various eclipse parties will be found among the articles. When these lines are read the results of the observations will be known to the world. We shall probably have learned much as to the nature of the corona, while the doubts which have been urged as to its position will (it may fairly be hoped) have been finally removed.

*The Zodiacal Light.*—Closely associated with the subject of the corona, the zodiacal light has received of late a considerable degree of attention. In a long paper on the subject in the Monthly Notices of the Royal Astronomical Society, Mr. Proctor discusses the various theories which have been propounded respecting this object. He remarks that the geometrical considerations applicable to the zodiacal light are too definite to admit of question—in other words, the path to be followed in seeking for a theory of the object is unmistakable; but he considers that hitherto this path has not been traced out far enough, "the perplexities which presently surround us as we follow it having seemed, perhaps, to render further research hopeless." The very difficulties of the subject, however, tend to render the rejection of erroneous theories more certain, and therefore must cause the true theory to admit of the more satisfactory demonstration. He then proceeds to discuss the several theories. He points out first that the rising and setting of the zodiacal light, in a manner precisely corresponding with what would be observed if it were a distant object like a planet or star, at once disposes of the theory that the light comes from matter lying within the limits of the earth's atmosphere. Such matter might seem, on a given occasion, to rise or set according to such a law, precisely as a balloon might seem to follow the motion of the setting sun; but only by a singular accident, and not systematically. Again, the theory that the light is due to a ring of matter surrounding the earth is disposed of by the fact that the gleam shows no appreciable parallactic displacement, as seen from different parts of the earth. Such a ring, if far off, would form always an all but complete arc of light, from the eastern to the western horizon; the shadow of the moon only appearing as a relatively narrow dark rift across the brightest part of the gleam. And if the ring were close by, it would be invisible in moderately high latitudes. Passing to cosmical theories, Mr. Proctor shows that the zodiacal light cannot be due to the existence of a disc of bodies, travel-

ling in orbits of small eccentricity around the sun; for in that case the luminosity of the gleam would be more constant, and its position more fixed, than is actually the case. Nor can the appearance, and changes of appearance, of the zodiacal light be accounted for by the existence of bodies travelling in orbits of considerable eccentricity, so long as the whole of each orbit lies relatively close by the sun. "We are thus led to the conclusion," he adds, "that the bodies composing the zodiacal light travel on orbits of considerable eccentricity, carrying them far beyond the limits of what may be called the zodiacal disc. The constitution of the disc thus becomes variable, and that within limits which may be exceedingly wide. They *must* be so, in fact, if all the recorded variations of the zodiacal light are to be accounted for. In other words, it is requisite (if the evidence is to be explained) that the paths of the materials composing the zodiacal light should be not only for the most part very eccentric, but that along those paths the materials should not be strewn in such a way that a given portion of any path is at all times occupied by a constant or nearly constant quantity of matter." According to this view, the constituents of the zodiacal light resemble very closely—at least, as respects distribution along their several paths, and the general figure of those paths—the meteoric systems which the earth traverses in the course of her motion around the sun. Mr. Proctor then proceeds to show in how many respects the results deducible from this theory accord with known facts respecting the zodiacal light, meteoric systems, comets, and the corona.

*The Colours of Jupiter.*—The planet Jupiter, which has of late formed so interesting a subject of study to astronomers, is now very favourably situated for observation—having passed his opposition on December 13. His colours are even more striking this year than they were in the winter months of 1869–70. Mr. Browning thus speaks of the appearance of the planet on October 24 and 25, 1870:—"The equatorial belt was of a fuller ochreish or tawny colour than when I observed it during the previous opposition. A bright belt to the north of the equator was much the brightest portion of the planet's disc. The dark belts on the northern side were of a very dark brown, with less copper colour in them than I found during my previous observations. The portion of the disc to the south of the equator was peculiarly free from belts. This refers specially to the views I obtained on the 24th. The hemisphere seen on the 25th had a light and a dark belt about midway between the south pole and the equator, tolerably prominent. The ochreish belt was mottled all over the surface with white cloudy markings or patches—a distinct line of them, though separated by darker markings between, evidently encircled the whole of the planet—a little way to the south of the true equator."

*Photographs of Jupiter.*—As some question has been raised by the Greenwich observers as to the reality of the changes detected and described by Mr. Browning, it is worthy of notice that, besides the abundant evidence which was brought forward in proof of Mr. Browning's views by telescopists who have observed the planet as well with refractors as with reflectors, photography has supplied a proof of singular force and clearness. Lord Lindsay has taken two photographic negatives of Jupiter at Mr. De la Rue's observatory, within a quarter of an hour of the time when Mr. Browning

made the first of the drawings described above. In these negatives the equatorial belt is almost absolutely transparent. As Mr. Browning remarks, "the light from the orange-coloured belt has failed entirely to act on the sensitive collodion surface." In negatives taken during previous years, the equatorial belt has exerted the most marked action on the collodion film, so that the belt has come out quite opaque.

*Browning's Automatic Spectroscope.*—The principle of this ingenious instrument has been made the object of several rival claims, which seem to us to have no foundation whatever. On the one hand, Herr von Littrow claims for his son (lately deceased) the invention of the plan, and in confirmation of the claim points to a volume of the Proceedings of the Imperial Society of Vienna. On reference to this volume we find an automatic spectroscope described, which bears not the most remote resemblance to Mr. Browning's, and would certainly not reward the mechanician who should attempt to remove it from the domain of pictures. On the other hand, Professor Young, in describing an instrument he has successfully employed himself, speaks of Mr. Rutherford as the originator of the idea of slotted bars working over a central pin. He mentions no date, however, nor does he give any evidence whatever to show that Mr. Browning had had the opportunity of hearing of Mr. Rutherford's ideas. Now Mr. Browning can prove that, so far back as 1862, the idea of his automatic spectroscope had not only been conceived by him, but described to others. As the automatic spectroscope is a most important addition to our spectroscopic appliances, it does seem desirable that those who advocate the claims of others to its invention should give satisfactory evidence in support of their views. But it may be remarked, in passing, that the mere enunciation of the idea that slotted bars attached to the prisms would give the required motion would by no means suffice to establish a claim as against Mr. Browning. A modification of the plan first described by Mr. Browning has been suggested by Mr. Proctor, and at the last meeting of the Royal Astronomical Society Mr. Browning exhibited a spectroscope constructed on this modified plan. It presents certain improvements. The theoretical requirements of an automatic spectroscope to give minimum deviation for all rays are strictly fulfilled, and, further, the motion of the viewing-telescope is guided by the same slot-movement which controls the motion of the several prisms. It is proper to point out, however, that these improvements must be regarded as essentially included within Mr. Browning's own plan. Mr. Proctor claims no share of the credit due to the modified instrument: and this is but simple justice, since surely nothing can be more unfair than to step in between an inventor and those improvements which are sure to follow the first construction of a new instrument, and then to claim the improved instrument as one's own.

*Double and Twice-acting Automatic Spectroscope.*—Mr. Proctor has proposed a plan, which we believe Mr. Browning is carrying out, for extending the automatic principle to a second battery of prisms, an intermediate prism of four faces carrying the light from one battery to the other. Of this prism three faces act on the light. At the first there is refraction, at the next total reflection, and at the third refraction again. As at each refraction the dispersion is increased precisely as when the light passes into or

out of any one of the prisms of either battery, there is no waste of light in the passage through the intermediate prism. The two batteries are automatically adjusted to minimum deviation for all rays. After passing through the two batteries, the light is reflected and sent again through both batteries, and by means of a diagonal prism the observer is enabled to examine the spectrum through an eye-tube at right angles to the axis of the collimator. In this last respect the plan differs in nothing from that adopted long since by Janssen and others. The novelty of the instrument consists in the arrangement for including a second battery in the automatic system. The dispersive power thus secured—with perfect adjustment—can be made as great as that due to twenty-one or even twenty-three equilateral prisms of dense flint-glass. As motion is communicated to the intermediate battery, the effective length of connection is no greater than in the case of the single automatic battery.

*Grubb's Automatic Spectroscope.*—Mr. Grubb has devised an automatic spectroscope, which is intended for use with the fine telescope lately placed by the Royal Society under the management of Dr. Huggins. In this instrument, which was devised so far back as last March, there are five compound prisms; each consisting of a rectangular prism of dense flint-glass, bounded by two prisms of crown-glass of small refracting angle, and introduced merely to get the light through the rectangular prism. These five prisms are automatically adjusted to minimum deviation, and the light is taken twice through the battery and viewed through a fixed eye-tube, as in Mr. Proctor's plan. Whether securing minimum deviation through a compound prism of this sort is equivalent to securing those optical conditions which result in the case of minimum deviation through a single prism, is a question which Mr. Grubb has not, so far as we know, considered. It seems unlikely that the spectral lines can be so well defined when these triple prisms are used as where single prisms are employed. But experience only can determine which plan is preferable.

*The Total Eclipse of the Moon last July.*—The Astronomer Royal observed this eclipse in a peculiar manner. "Being shortsighted," he says, "(the distance for distinct vision less than five inches), and every luminous object being seen as a broad blur, I can compare the quantity of light from sources of very different character. Thus I found that the quantity of light received from the eclipsed moon was less than that from Saturn and greater than that from $\alpha$ Aquilæ, including some from $\beta$ and $\gamma$ Aquilæ. The light of the moon increased considerably in ten minutes after the time of central eclipse." "I infer," he adds, "that the only part in which the shadow is nearly total is the centre of the shadow, and that a large amount of light falls within the geometrical limits of the true shadow."

*The Satellites of Uranus.*—A question has been raised as to the existence of any more satellites of Uranus than the four seen by Mr. Lassell. Professor Pritchard has indeed somewhat roundly asserted that to believe in more than these four is to attack Mr. Lassell. Very few seem to be aware how the matter really stands. Mr. Proctor, in the Monthly Notices of the Astronomical Society, defends the claims of Sir William Herschel's other four satellites. He points out that Sir William Herschel by no means expressed (as the Savilian Professor supposes) a mere suspicion of the existence of these

other satellites. On the contrary, Herschel distinctly says that he had not kept back the announcement on account of any doubts he had, but because he hoped to determine the elements of the orbits. Now it has never happened that where Sir William Herschel has expressed certainty about a matter of observation, he has been shown eventually to have been in error. But it may be urged that Mr. Lassell's telescope being far finer than Herschel's, and showing the four other satellites far more distinctly than they seem to have been seen by Herschel, the non-detection of the four fainter ones would seem to show that they have no existence. Here, however, a distinction must be drawn between two qualities of a telescope—defining and illuminating power. It is quite clear that as respects defining power Mr. Lassell's four-feet reflector has greatly the advantage over the four-feet reflector at Slough; and as respects the satellites nearest to the planet, he would on this account alone probably have a certain advantage, since an ill-defining reflector would throw a good deal of diffused light over the field near the planet. In illuminating power, however, it seems by no means so clear that the Slough reflector is greatly surpassed by Mr. Lassell's, even under equal circumstances. Now Sir William Herschel failed to detect the satellites when he used his telescope as Mr. Lassell has always used his—that is, as a Newtonian; and only when he used the "front view," by which the illuminating power was greatly increased, could he gain even a glimpse of them. Even then only his skill and long practice in searching for minute points of light enabled him (as he himself tells us) to succeed. Under these circumstances there seems no adequate reason for dismissing these four satellites from the solar system. It is worthy of notice that Mr. Lassell does not seem to have been aware, when he urged the dismissal of the satellites, that Herschel had had more than a suspicion of them; for he says "Herschel may have mistaken small stars for satellites," the very error the great astronomer was so careful to avoid. So far as one of the satellites in question is concerned, indeed, Mr. Lassell has himself shown (unwittingly) that it may exist. For he has said that *if* any satellites within the range of his telescope exist, they must travel very far from their primary, and in a period of at least three months. Now one of Herschel's is at a distance corresponding to a period of more than 3½ months.

*Photographing a Solar Prominence by Means of the Spectroscope.*—Professor Young has succeeded in taking a photograph of a solar prominence by means of a double-acting spectroscope of high dispersive power. The result is in itself, as he admits, valueless; but it is full of promise. The day will doubtless come when the condition of the sun's limb will be recorded as systematically by photography as the condition of the solar surface has been recorded at the Kew observatory.

*The Planets.*—Jupiter will continue favourably situated for observation during the next quarter. He is stationary on February 10, and in quadrature on March 8. Mars, however, is the planet of the quarter, as he comes to opposition on March 20. He will be stationary on February 9. As he will be in aphelion on January 21, this is not a favourable conjunction. But the northern parts of his surface ought now to be studied, as they are more fully turned towards the earth than in perihelion oppositions. Saturn is slowly

passing away from the sun's neighbourhood, and will not be stationary until April 19.

*Eclipse of the Moon.*—On the evening of January 6 the moon will be partially eclipsed. The following are the principal elements of the eclipse (which will be visible in England):—

| | Mean Time at Greenwich H. | M. |
|---|---|---|
| First contact with the penumbra | 6 | 27·3 P.M. |
| First contact with the shadow | 7 | 46·2 „ |
| Middle of the Eclipse | 9 | 16·4 „ |
| Last contact with the shadow | 10 | 46·6 „ |
| Last contact with the penumbra | 12 | 5·5 „ |

The magnitude of the eclipse (moon's diameter as 1) will be 0·688. First contact with the shadow will occur at 130° from the northernmost point of the limb towards the east; and last contact at 127° towards the west. (In each case for *direct* image.)

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## BOTANY.

*"Crow's Nests" or "Fasciations."*—At the meeting of the Academy of Natural Science of Philadelphia in August last, Mr. Thomas Meehan said very little had been written about the causes of those bunches of branches often seen in trees, and called by the people "crow's nests," and by botanists *fasciations*. Dr. Masters, in *Teratology*, briefly refers to them, and alludes to "over-nutrition" as the cause of their existence. He had watched almost daily the past year one of *Abies balsamea* on his own grounds. The branchlets were weak, the leaves were comparatively long and slender, not distichously arranged, pale in colour, deciduous, and many of the branchlets died in the winter. All these were evidences of weak nutrition. He had found two trees of sassafras, apparently of the same age, growing within a few yards of each other, but one with numerous fasciculated bunches. In addition to the characters in the other case, here the fasciculated tree was not as large as the other one. That weakness, not strength, was the cause, was also proved by facts from an opposite direction—namely, the law of sex. He had already shown that a low condition of vitality favoured male, at the expense of the female organs. He had found a large number of fasciculations in the common blackberry, and in all instances, besides the yellowness and the other marks, there was a tendency to abortion in the pistils, an increase in the number of petals, and a development of foliaceous points to the usual calyx segments. So that his law of sex, as well as the usual phenomena of weakened vitality, indicated that it was this and not over-nutrition which caused fasciations in trees.

*The Germ Theory of Fermentation.*—A series of papers under this heading has recently been published in the *Chemical News* by Dr. A. E. Sansom. They can be referred to by those not well acquainted with the literature of the subject. We find, however, nothing that is new in Dr. Sansom's observations.

*The Fungus Show at the Royal Horticultural Society* on Wednesday, Oct. 5, was an attractive one. Notwithstanding the drought, a great many fine and curious specimens were exhibited; and as there was no collection in the first class, which was confined to edible fungi, the prizes were awarded to Mr. G. Worthington Smith and Mr. James English, whose collections were considered of equal merit, while the second prize was given to Mr. Hoyle.

*The Fertilization of Salvia and other Plants.*—Mr. Thomas Meehan called the attention of the American Association to the arrangements of some plants for preventing fertilization through any other than insect agency, as discovered by Darwin. The *Salvia* family of plants had the most elaborate arrangements for insect agency, but it had been objected to Darwin's theory that insects made no use of them. Bees bore holes through the tube from the outside for the honey, and do not enter by the mouth of the flower, as they ought. In the same way, in the *Petunia*, bees bore for honey from the outside. He had discovered that in these cases, where day insects failed to make use of these apparatuses, fertilization was carried on by night-moths, so that the objections to Darwinism were removed. He also referred to the common sweet chestnut, as bearing two classes of male flowers, only one of which probably aided in fertilization. The first class appeared ten days before the other, and are those which give whiteness to the trees. They appear in the axils of the weak shoots. The female flowers appear on the apices of strong shoots, according to his theory of the laws of sex. The second class of male flowers appear at the ends of the vigorous shoots bearing the female flowers. Whatever affects the vigour of the tree interferes with the production of female but not of male flowers, and this was the reason why some seasons had short crops.

*The Venation of Hawthorns.*—In a paper read before the Microscopical Section of the Manchester Literary and Philosophical Society (Nov. 7), Mr. Charles Bailey says:—"It is not a little remarkable that there is one peculiarity in the venation of the hawthorns which is invariably overlooked by the draughtsman and engraver, viz. the direction of the secondary nerves, which proceed from the midrib to the base of each sinus; such an arrangement is very rare, being found only in some other species of *Cratægus*, as *C. Azarolus*, &c., in species of *Fagus*, and in a few other plants."

*Are certain Botrychiums Epiphytic?*—Mr. J. H. Redfield states that on a recent visit to the northern part of the State of New York, he had noted the *Botrychium lunarioides* and *Botrychium lanceolatum* growing under circumstances that seemed to confirm the idea that these species are really underground parasites, or *epiphytic* plants. More than twenty plants were noticed scattered over a space of a mile in length, and in every instance they were growing near the common blackberry (*Rubus villosus*), and every plant that was lifted had its roots in contact with the root of the blackberry. He referred to the peculiar character of the root of this genus—so different from that of other ferns, and so similar to that of some orchids—and to the fact that these species, so widely distributed, seem nowhere abundant—as favouring the idea of their epiphytic character. Mr. Newman some years ago expressed the opinion that the British *Botrychium lunaria* is an underground parasite, but Moore and others have doubted. Mr. Redfield desired to call the attention of botanists to the conditions under which these and other

species of *Botrychium* may be found, with a view to determine the question.

*The American Compass Plant.*—Dr. Thomas Hill, who read a paper on this subject before the American Association at the last meeting, says that in June, 1869, as he was coming from Omaha to Chicago, on a very dark rainy day—so dark that he could not form any estimate of the points of the compass from the sunlight—at three different points on the prairies he noticed young plants of *Silphium laciniatum*, and estimated from them, while going at full speed, the course of the railway track. On reaching Chicago he procured, by the kindness of the officers of the C. & N. W. road, detailed maps of the track, and found where he had estimated the bearing at 35°, 75°, and 90°, the true bearings were 31°, 78°, and 90°. In October, 1869, being detained by an accident at Tama, he gathered seed, and this spring raised a few seedlings. Drought and insects destroyed part of them, and he could only give the history of eight plants, with fourteen leaves. Ten of these fourteen leaves showed a strong disposition, when about four inches high, to turn to the meridian; the other four showed a feeble disposition in the same direction. These ten leaves, on coming up in June, had an average bearing of 42°, and the mean bearing was nearly as large. But in August the same ten leaves showed an average bearing of only 4½°, and the mean bearing was but 2½°. Dr. Hill refers this polarity to the sunlight, the two sides of the leaf being equally sensitive, and struggling for equal shares. He hoped in a more favourable summer to test this, and several other points which had suggested themselves, by experiments.

*Scleroderma vulgare an Eatable Fungus.*—The *Food Journal* (an interesting periodical, which we are glad to see succeeding) contains a very interesting note on the above by the Rev. M. J. Berkeley, F.R.S., in the December number. It is as follows:—"I was somewhat surprised some time since, amongst a host of other enquiries respecting the qualities of particular fungi, to have *Scleroderma vulgare* submitted to me; and still more so, after my evil report of it, to find that it had been largely eaten and pronounced very good. It is only in the young state, of course, that any question could arise about it, for like its allied puff-balls, when old, it is filled with a mass of loathsome dust. I ought, however, to have recollected that its use as an article of food was no novelty, as under another and false name it has been largely employed at Paris instead of the truffle of Périgord, to adulterate Périgord pies, the quality of which was, in consequence, much deteriorated. Young specimens were given by the late Monsieur Desmazières in his 'Plantes Cryptogames du Nord de la France,' Fasc. xvi., 1836, as specimens of the white truffle, though the structure is totally different. It is found abundantly in the neighbourhood of Mons, where it frequently appears in the market, and is sent from Belgium, in great quantities, to Paris. Some pains are taken to guard it in the place of its growth, by covering it with earth, until of sufficient size, against the ravages of animals, but especially of the magpies. The same thing clearly is figured by Corda, under the name of *Pompholyx sapidum*, who considers it superior to either the black or white truffle. I am not certain whether Dr. Bull, who is such an authority on the subject, has really tried its culinary properties; but in a letter, recently received, he says, 'I am afraid *Scleroderma vulgare*, though doubtless edible when

young, like all the other puff-balls, will scarcely do to *recommend* as edible, since it is so small and not attractive by its colour, even when young, and very dangerous at an earlier stage.' There seems always to be conflicting evidence about the quality of fungi, the truth probably being that the same species may be wholesome or the contrary, according to local or climatic condition, or from idiosyncratic peculiarities of constitution. *Sparagris crispa*, one of the largest and most beautiful of our fungi, has occurred lately in two or three localities; and I have myself had some dressed which came from Miss Broadwood, of Lyne, in Hampshire, which proved excellent. A single specimen, like *Lycoperdon giganteum*, is so large that it would almost be sufficient for a Lord Mayor's feast."

*Nutrition and Sex in Plants.*—In the *American Naturalist* for November, Mr. Thomas Meehan gives a short account of his paper on the above subject. He refers to his "laws of sex," read last year, and now proposes to show that a decreased power of nutrition is one of the operating causes against that high state of vitality necessary to produce the female sex. He stated that there were two classes of male flowers on the common chestnut (*Castanea Americana*), one from the axils of leaves on weak branches, the other terminating the vigorous shoots, only on which the female flowers are formed. The axillary male flowers mostly matured before the supra-pistillate ones opened. These were extremely weak, owing to the superior absorptive power of the females below them. He then exhibited some specimens of these, as well as some from a very large chestnut tree, which had always borne abundant fruit, but had this year produced nothing but male flowers. The leaves were all striped with yellow and green, indicating, as every experienced gardener knows, that nutrition was obstructed. Plants over watered, by which the young feeding roots rotted, always put on this yellow cast. The yellow tint always followed "ringing" the branches, or any accident done to the bark. The influence of this *defective power of nutrition*, in this instance, he held so clear that he had no difficulty in concluding that it was *one of the agents which operated on the laws of vitality that governed the sexes.*

*Difference of Sex with Difference of Station.*—It is a very curious fact that there are many plants common to this country and America which present different sexual characteristics in the two. At a late meeting of the Philadelphia Academy of Science, Mr. Meehan exhibited some specimens of *Rumex oblongifolius*, a naturalized dock from Europe. He said that so far as he could ascertain from European specimens, and the descriptions of Babington, Bromfield, and other English botanists, the plant was there hermaphrodite; but there, as correctly stated by Dr. Asa Gray, it was monœciously polygamous. He thought the fact that plants hermaphrodite in one country becoming unisexual in another was worthy of more attention by those engaged in the study of the laws of sex than had been given to it. This *Rumex* did not stand alone; *R. crispus* and *R. patienta* exhibited the same thing. *Fragaria* was another instance well known to horticulturists, although the fact scientifically had not received due weight. The average tendency of the strawberry in Europe was to hermaphrodism—here to produce pistillate forms. He also called attention to the fact that in these American specimens unisexuality was in proportion to axial rigor. This law

he had already explained in times past to the Academy, and new instances were scarcely necessary. Here, however, the moderately weak plant had more hermaphrodite flowers than the strong one; and in both classes of specimens the number of male flowers gradually increased with the weakening of the axis, until the ends of the raceme were almost wholly of male flowers. The first flowers on the strong verticels were usually wholly pistillate.

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## CHEMISTRY.

*The Filtration of Strong Acids.*—Dr. James St. Clair gives the following simple and exact method in the "Chemical News" of September 30: "Into the narrow part of an ordinary glass funnel, spun glass (such as is used in making tails for glass birds) is closely packed, and over this is sprinkled ground glass to the depth of a quarter of an inch, care being taken that both the funnel and glass are perfectly clean. About four ounces of boiling water are then allowed to pass through the filter, which is then allowed to dry, and, previous to being used, is moistened with a pure specimen of the acid to be filtered. A filter so constructed is as efficient as any with which he is acquainted, is very durable and cheap, while, from the fact that these acids do not act on glass, freedom from contamination during the process is perfectly ensured. Such filters, or the materials necessary for their preparation, may be obtained at Mr. Motherwell's, 73 Union Street, Glasgow."

*What is Thymol?*—The "Medical Press" of December 7 says that Mr. Henry Draper, of Dublin, exhibited to the Dublin Chemical Club, at its last meeting, a specimen of a new preparation which has been proposed as a substitute for carbolic acid. It is named thymol, and is a derivative of the *Thymus vulgaris*, the monarda or horse-mint, and the Ptychotisan East Indian umbelliferous plant. It is of a similar chemical composition to carbolic acid, but destitute of the very unpleasant smell of this popular disinfectant. It melts at 44° Centigrade, and is soluble in 300 parts of water. It resembles carbolic acid in forming compounds with potash and soda, but differs from it in that these compounds are very unstable, being decomposed even by carbonic acid. The introduction of this preparation recalls to mind the fact, that oil of thyme was in past years a favourite popular remedy for the toothache, and it is only now that its efficacy and the causes of such efficacy have been made manifest. The oil of thyme is prepared in large quantities in the south of France, where it is used for printing on china.

*Acid Nature of the Organic Matters in River Water.*—Herr F. Stolba, writing in the second number of "Dingler's Journal" for October, states that, according to his experience, obtained by making (in Bohemia) a large number of water analyses, all waters which contain a large proportion of organic matter contain it in combination with bases, chiefly lime, and that, therefore, the organic matter is (as already suggested, and partly experimentally proved by Berzelius) of an acid nature.

*Death of Professor Miller.*—The "Chemical News" gives the following sketch of Professor W. Allen Miller, M.D., F.R.S., Professor of Chemistry in King's College, who died on October 30, 1870. He died at Liverpool,

whither he had gone to take part in the proceedings of the British Association. Dr. Miller was born at Ipswich, on December 17, 1817, and in his twenty-fourth year he became assistant to the late Mr. Daniell, Professor of Chemistry in King's College, London. In 1844, he co-operated with his master in the publication of a paper on the "Electrolysis of Secondary Compounds." In the following year he was elected a Fellow of the Royal Society, and succeeded Mr. Daniell in the chair of chemistry in King's College. His chief work at this time was his paper on the "Spectra of certain Vapours," published in 1845. In 1849, he again came before the scientific world with a paper on the "Atomic Volumes of Organic Liquids." From this date his time appears to have been chiefly absorbed by other than purely scientific subjects. He held the posts of treasurer of the Royal Society, president and afterwards vice-president of the Chemical Society, and assayer to the Royal Mint, besides being member of the Science Commission. His later contributions to the scientific periodicals were, a paper on "Transparency," in the "Journal of the Chemical Society," some "Analyses of Gutta Percha," and "A Treatise on Potable Water." In conjunction with Mr. Huggins he investigated the spectra of the fixed stars. He is known to the educational world by his voluminous and widely popular "Treatise on Chemistry," in three parts, which originally appeared from 1855 to 1857, and which has already gone through several editions. Although Professor Miller was not a member of the Society, he took an active interest in its operations, and served on several of its committees.

*The Theories of Fermentation.*—In the "Bayerisches Industrie- und Gewerbe-Blatt" (August), Herr Dr. A. Weinberg gives a very exhaustive paper on the above subject. It was originally a lecture before the Chemical Society of Munich, and deals as follows with the several investigators who deserve to be named in connection with the subject: 1. Those who consider fermentation to be a purely chemical process, the result of the chemical action of the ferment or yeast on the sugar; MM. Trommsdorff and Meissner are the founders of this theory. 2. Those who consider fermentation to be a process of galvanic decomposition, called forth by the dualism of the exciting body in a conducting fluid; this theory was founded by M. Kämtz, and among its adherents are MM. Schweigger, Colin, and Kölle. 3. Those who consider it as a catalytic process, or as due to the action of porous bodies; this theory was founded by M. Berzelius. 4. Those who consider that fermentation is due to the action of certain nitrogenous matter, which is itself permanently in a state of decomposition, which is imparted to the sugar as soon as it (the sugar) comes into contact with the decomposing nitrogenous matter under favourable conditions—the consequence being the splitting up of the sugar into alcohol and carbonic acid; this view has been established by Dr. von Liebig, and is adhered to, among others, by MM. Frémy, Löwig, Gerhardt, &c. 5. Lastly, fermentation is viewed as being a kind of process of vegetation, the newly-formed yeast being considered as the newly-generated plant; this view is held by MM. Erzleben, Cagniard-Latour, Schwann, Dumas, Mulder, and others.

*The Manufacture of Iodine*, according to Professor Wagner, already amounts to 30,000 pounds a year.

*Analysis of Birmingham Water.*—Dr. Hill read a paper before the British

Association, which is reprinted by the "Food Journal" for December. The revelations then made as to the qualities of the shallow well-waters are such as to shock the feelings of any thoughtful man. The table is too long to quote in full, but a few facts from it will suffice. The total solid impurities per gallon figure in enormous amounts, such as 256, 380, and even 507 grains per gallon; while the organic contamination runs through a rising scale till it reaches the almost incredible figure of 48·16 grains per gallon, in the very water holding 507·02 grains of solid matter in each gallon.

*Composition of the Water of the Nile.*—The water of the Nile has recently undergone investigation by Herr O. Popp ("Annalen der Chemie," September). The sample of water taken for analysis was obtained from the middle of the river, some six miles below Cairo. Previous to being analysed, the water was left standing for two days, after which time the water was first filtered; but, even after this operation, it did not become quite clear, and it was found necessary, consequently, to leave it standing for some few days longer, when it deposited a flocculent sediment, which, on being tested, was found to consist of silica, a minute quantity of organic matter, lime, and magnesia salts. One litre of the water contains, in grammes weight—carbonic acid, 0·03146; sulphuric acid, 0·00390; silica, 0·02010; phosphoric acid, 0·00054; chlorine, 0·00337; peroxide of iron, 0·00316; lime, 0·02220; magnesia, 0·01467; soda, 0·02110; potassa, 0·00468; organic matter, 0·01720; total, 0·14238 grm. Percentage composition of dry residue—carbonic acid, 22·155; sulphuric acid, 2·755; silica, 14·150; phosphoric acid, 0·379; chlorine, 2·372; peroxide of iron, 2·227; lime, 15·640; magnesia, 10·332; soda, 14·852; potassa, 3·300; organic matter and small quantity of ammoniacal salts, 12·025; total, 100·187.

*Detection of Sulphur in Coal-Gas.*—In a recent number of the "Journal für Gasbeleuchtung," Herr Ulex gives the following method. Let a platinum basin be filled with half a litre of water, and the basin be heated over a Bunsen-burner until all the liquid has evaporated; the basin will be found to be coated, on the outside, where it has been struck by the flame, with a dirty, greasy looking substance, which, on being washed off with pure distilled water, and tested, proves to be sulphuric acid. The author further points out that the glass chimneys used with Argand gas-burners soon become coated over internally with a white substance, which, on being washed off with distilled water, will be found to be, on testing, sulphate of ammonia. The glass panes of a room wherein gas is burned for a few evenings consecutively will, when rubbed with the fingers of a clean hand, impart to it a substance which, on the hand being rinsed in distilled water, will yield a precipitate of sulphate of baryta with chloride of barium, and a brick-red precipitate with potassio-iodide of mercury.

*Powdering Camphor.*—In the "American Journal of Pharmacy" for November, Mr. W. Proctor gives some hints as to the above. It is well known that camphor is easily reduced to powder by rubbing with a few drops of alcohol, but the powder so made will, after a short time, aggregate to crystals, which have to be rubbed down again. The author mixes with the powder of camphor so obtained, carbonate of magnesia, 10 grains to the ounce being sufficient; this powder never cakes or forms crystals.

*A Sensitive Test for Hyposulphites* is given by Dr. Boëttger in the "Journal

für prakt. Chemie" (No. 13, 1870).—Dissolve, he says, 1 decigrm. of very pure permanganate of potassa, and 1 grm. of perfectly pure soda (prepared from sodium) in half a litre of distilled water; there is thus obtained a perfectly red-coloured liquid, which, upon the addition even of a very minute trace of any hyposulphite, becomes, at once, green. This test, the author states, is so delicate, that traces of a hyposulphite present in neutral sulphites may thus be detected.

*Loss of Weight in Platinum Crucibles.*—In "Dingler's Journal" for October, which is considerably abstracted in the "Chemical News," Dr. F. Stolba is stated to have made a number of experiments on this subject, the results being that the rougher and more unpolished the surfaces of platinum crucibles are, the more readily they are acted upon by the flame of well-made Bunsen-burners, because the rough unpolished surface promotes the formation of a compound of carbon and platinum, which is partly consumed by the flame, partly mechanically carried upwards in the hot current of air, after having been detached from the crucible. The author found, by experiment, that, when a good platinum crucible was heated for twelve hours continually, it lost 0·016 grm. in weight, and its surface appeared as if etched, or as the well-known *moire métallique.* This loss and alteration are not due to the presence and subsequent elimination of osmium, because the loss is greater of the crucible than the quantity of osmium contained in crude platinum. The brighter and more polished the surface of platinum crucibles and other platinum vessels exposed to ignition is kept, the better they will resist the deleterious action of the flame.

*What is Ccollpa?*—By this name Herr Schickendantz understands (says the "Chemical News") a saline efflorescence, not unfrequently met with in certain parts of the slopes and along the rivers originating in the Cordilleras de los Andes. The author gives ("Annalen der Chemie," Sept.) at great length, details of the analysis of several samples of this substance, which, setting aside impurities present only in small quantity, consists, in 100 parts, of—soda, 45·21; carbonic acid, 28·90; water, 25·70; agreeing nearly with the formula $Na_4C_3O_8 + 2H_2O$. Another sample consisted mainly of—water, 35·28; soda, 38·28; carbonic acid, 26·44; nearly agreeing with the formula $Na_4C_3O_8 + 3H_2O$. The author resides at Pilciao, province of Catamarca, Argentine Republic.

*Influence of Light on Petroleum.*—According to the researches of M. Grotowski (published in a recent number of the journal which the siege of Paris has cut off from us, "Cosmos"), when the petroleum oils are exposed to the solar light under certain conditions, they absorb a certain quantity of oxygen, and convert it into ozone in a similar manner to what has been observed in connection with other hydro-carbons. The oxygen does not seem to combine with the oil, but reacts energetically as an oxidizer upon substances with which it is brought in contact, thus the cork of the vessels in which it is contained is generally acted upon to some considerable extent. After the action of the ozone, the oil boils with difficulty. The colour of the vessels in which the oil is placed has a great deal to do with the absorption of the oxygen.

*What is the Active Principle of Ricinus Seeds?*—Herr E. Werner has taken up this subject since Dr. Tuson's papers have been published, and after referring

at length to Dr. Tuson's researches, he describes a series of experiments, chiefly made with the view to obtain, from the ricinus seeds, an active principle suitable for medicinal use. As regards the ricinine of Dr. Tuson, prepared by the author in large quantity and according to Dr. Tuson's directions, it is stated that ricinine is not an alkaloid, and, moreover, a substance which contains a considerable quantity of ash; and the author, after carefully made analysis, comes to the conclusion that Dr. Tuson's ricinine is a compound of magnesia and of an organic acid, the formula of this body being $C_{11}H_{20}O_{10}Mg_2 + 6H_2O$. This abstract has been published in the valuable collection of the "Chemical News." It appeared originally in the "Pharm. Zeitschrift für Russland," No. 2, 1870.

*Bromine in Large Quantities.*—The "Boston Journal of Chemistry" for November gives us some facts of interest. The writer says that five years ago Bromine was sold in this country and in Europe as high as eight dollars a pound; now the price is less than a dollar and a half the pound, and the consumption has increased in a thousand-fold ratio. It says: "As a manufacturer of chemical substances, we did not have occasion to purchase for manufacturing purposes twenty pounds a year until after 1865, when a great demand sprang up for the bromides of potassium, sodium, and ammonium. Some idea of the increase in consumption may be formed from the statement that we have ordered of the salt-makers in Pennsylvania quantities as large as five thousand pounds, or *two and a half tons*, at one time, during the past year. Our bromine supply formerly came from Germany, the Stassfurt salt-mines furnishing it in considerable quantities after they were opened; but now our own strong salines in Pennsylvania, Ohio, and West Virginia produce it in amounts fully equal to the demand."

*The Excrements of Egyptian Bats.*—Herr O. Popp has published a curious paper, which is also reported by the "Chemical News." After referring to the curious fact that Egypt, owing to its very clear sky at nights, and its sub-tropical climate, is especially suited for bats, of which no less than eight different *genera* are found there, the author proceeds to detail the methods of analysis at length; the result of the composition of the excrements, in 100 parts, being: urea, 77·80; uric acid, 1·25; kreatine, 2·55; phosphate of soda ($2NaO,HO,PO_5$), 13·45; water driven off at 100°, 3·60; substances insoluble in water, 0·575; total, 99·285. In a foot-note to this paper, Dr. F. Wöhler states that very recently the excrements of bats from Egypt have become an article of trade, as a sort of guano for manure purposes.

*Water in Edinburgh.*—Upon this subject Dr. Alexander Wood reports as follows:—"1. That the Heriot, the Talla, and St. Mary's Loch all afford water of a quality suitable for all the purposes for which it is required in a town. 2. That the Heriot is a better water for general domestic use than the Talla. 3. That the spring water of the Heriot and the Talla is superior to the lake water of St. Mary's. 4. That the construction of the necessary ponds for storing the produce of these springs would go far to deprive them of any superiority which they at present possess, and would certainly render the water supplied from such ponds inferior in some respects to that obtained from the natural lake. 5. That the analysis of the water of St. Mary's Loch shows it to contain a sufficient quantity of the salts of lime to remove all

fear of the danger suggested in the letter of 'A Physician, especially when the copiousness of the supply of these salts from other sources is considered. 6. That, under these circumstances, it appears to me that the water procurable from any one of the three sources of supply being suitable, the trustees should be guided in the selection by the questions of quantity, engineering difficulties, and comparative expense, and not by the opinion of any physician. 7. That the present supply of water in Edinburgh is manifestly insufficient, and that the poorer classes especially are not receiving enough to maintain them in a healthy state. 8. That should any epidemic disease appear among us, they will be less able on this account to resist contagion, or to bear up against disease if attacked."

*Derivatives of Anthracene.*—The meeting of the Chemical Society on December 1 was altogether taken up by Mr. Perkin, F.R.S., who read a very lengthy and important paper on the above subject. It is very long and impossible to abstract. It was a detailed account of some of the Anthracene derivatives, more particularly of the products resulting from the action of sulphuric acid on Dibrom- and Dichloranthracene. It is fully reported in the "Chemical News" of December 9.

*Chemical Chairs. Appointments.*—The chair of St. Bartholomew's, vacant by the decease of Dr. Matthiessen, has just been given to Dr. Russell, lecturer on chemistry in St. Mary's Hospital. The chair at King's College, vacant by the death of Dr. Miller, has been awarded to Dr. Bloxam, who formerly held the chair of practical chemistry in the same school.

## GEOLOGY AND PALÆONTOLOGY.

*Beds of Bog-Iron.*—At the meeting of the American Association, Professor A. Winchell presented a brief note on the above subject. It related to the occurrence of enormous beds of bog-iron in the upper peninsula of Michigan, on the tributaries of the Monistique river. It occurs in a half desiccated bog covering several townships. It is of remarkable purity, and of great but unknown depth. It lies directly in the track of the projected railroad, intended to connect the North Pacific Railroad with the railroad system of Michigan. The ore can be floated down the Monistique and its tributaries, to Lake Michigan, in the immediate vicinity of an excellent harbour. This immense deposit is undoubtedly derived from the disintegration of the hæmatites and magnetites of the contiguous region on the West. The ore will possess great value for mixing with the other Lake Superior ores.

*Geology and Agriculture.*—Owing to the Agricultural Society having obtained the services of Mr. Jenkins, its Reports have been more scientific than they were, and this is a circumstance that the Society is to be congratulated upon. In the last number of the "Journal of the Royal Agricultural Society" [1870, 2nd series] there is a Report on the Farming of Monmouthshire, by Mr. W. Fothergill, which contains a small geological map of the county, and also an account of the soils found upon each formation. The Devonian rocks furnish a great variety of soils. There is a deep

loam especially favourable to the oak and the apple: the conglomerates furnish soils well suited for roots and barley; on the clay a strong wheat-soil is formed, whilst the slaty or shaly beds are best adapted for woodland. The mountain limestone soil produces good pasture for the native sheep and cattle. The millstone grit appears adapted for sheep-walks only. It has been stated, says Mr. Fothergill, that the worst land in England lies upon the coal-measures; and certainly, at its best, it is but hungry soil; however, by draining and liming, it may be rendered in a measure productive. The new red sandstone is of well-known and marked fertility, producing in rich abundance every kind of crop. The lias furnishes a cold, wet, tenacious clay or clayey loam. When pervious it is found fit for cultivation, and grows good wheat and tares. The alluvium gives great variety of soil, both in appearance and in productiveness; and this arises partly from deficient drainage, partly from the character of the subsoil, and sometimes from the elevation.

*The Jurassic of France and the Oolitic of England.*—The relation of these two systems in two distinct countries has been well traced by Dr. Thomas Wright, who shows that there is a wonderful correspondence between them. The subject is noticed at some length in the "Geological Magazine." The localities taken are the Jurassic department of the Côte-d'Or of France and the Oolitic of Gloucester and Wilts, England.

*The Former Existence of Local Glaciers in the White Mountains of America.*—At the meeting of the American Association for the Advancement of Science, held in the autumn, a very interesting paper by Professor Agassiz was read by Mr. J. Perry on the above subject. The author considers that twenty-three years ago, when he first visited the White Mountains, he discovered unmistakeable evidence of the former existence of glaciers. His paper is published in the "American Naturalist," and deals at considerable length with the subject.

*Indisposition of Sir R. Murchison.*—There are very few scientific men, whether geologists or not, who will not hear with regret of the serious indisposition of Sir Roderick Murchison. His age indeed is ripe, but his energies are active; and hence his loss to British geology would not be one whit worse than his loss to the geologists of the British Isles.

*The Carboniferous Flora of Bear Island.*—On November 9 a paper by Professor Oswald Heer was read before the Geological Society "On the Carboniferous Flora of Bear Island" [lat. 74° 30′ N]. The author described the sequence of the strata supposed to belong to the carboniferous and Devonian series in Bear Island, and indicated that the plant-bearing beds occurred immediately below those which, from their fossil contents, were to be referred to the mountain limestone. He enumerated eighteen species of plants, and stated that these indicated a close approximation of the flora to those of Tallowbridge and Kiltorkan in Ireland, the Greywacke of the Vosges and the southern Black Forest, and the *Verneuilii*-shales of Aix and St. John's, New Brunswick. These concordant floras he considered to mark a peculiar set of beds, which he proposed to denominate the "Ursa-stage." The author remarked that the flora of Bear Island has nothing to do with any Devonian flora. The paper gave rise to considerable discussion.

*North of England and South of Scotland Silurians.*—Dr. W. A. Nicholson

read a paper on the above before the Edinburgh Geological Society on November 17. He pointed out the resemblances which he thought might be laid down between the above and the silurians of the North of England. He drew attention to the fact that the Wrae limestone of Peeblesshire might very possibly be the equivalent of the Bala limestone of Wales, and the Coniston limestone of Cumberland and Westmoreland; and that this would considerably simplify the elucidation of the Scotch silurians. He showed, however, that much work must yet be done before it would be possible to speak with any certainty as to the correlation of these ancient deposits.

*Ancient Earthquakes.*—In the "Geological Magazine" for December, Mr. J. Prestwich, the President of the Geological Society of London, gives a curious record of the above, collected from the writings of his relatives. According to this record, the earthquakes were distributed as under by centuries and seasons.

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| | | January | 2. | | |
| 11th Century, | 6. | February | 3. | | |
| 12th ,, | 16. | March | 2. | | |
| 13th ,, | 6. | April | 4. | Winter months | 11. |
| 14th ,, | 3. | May | 5. | | |
| 15th ,, | 1. | June | 0. | Spring | 11. |
| 16th ,, | 5. | July | 3. | | |
| 17th ,, | 9. | August | 4. | Summer | 7. |
| 18th ,, | 13. | September | 6. | | |
| | — | October | 1. | Autumn | 8. |
| Total | 59. | November | 1. | | |
| | | December | 6. | | |

This shows a great prevalence of earthquakes in the 12th century, a gradual decrease in numbers to the 15th, and then a gradual increase to the 18th century. They seem also to have been more numerous in winter and spring than in summer and autumn. The months, however, in many cases are not recorded, and no doubt the general record is confined to the more important and noticeable catastrophes.

*Mediterranean Geology.*—Mr. G. Man, in the last number of the "Geological Magazine" under the title of "Notes on the Mediterranean," gives a great deal of interesting information. Among other points he refers to the well known Temple of Serapis. This, he says, giving evidence of submergence and re-elevation within known heights and certain limitations of historic time, suggests a comparison with other evidences of similar elevation; and he would remark that Lyell's estimate of the amount of emergence of the shell-bored columns on the Italian coast agrees almost exactly with the amount of elevation in other distant parts, implied by the raised gravel ridge on the Corsican marshes, the great plain of shingle at the Rhone delta, and the lagoons of the south-western Mediterranean French coast. Furthermore, Mr. Gwyn Jeffreys informs him that a recent marine deposit, containing *Galeomma Turtoni* and other shells of species now living in the adjacent parts of the Mediterranean, occurs at Antibes at a similar height, viz. 25 feet, above the sea-level; and Mr. James Smith attributes the sandy plain immediately to the north of Gibraltar to a period of stationary level with a littoral zone exactly 24 feet above the

present level of the sea. The close coincidence in height is remarkable, and if these elevations at such distant points were contemporary, it implies a uniform amount of oscillation over a very large proportion of the Mediterranean area. The raised coast deposits at Tangier and Cadiz may have been connected with an independent oscillation of greater antiquity than the 25 feet rise, but yet more recent than that which submerged and re-elevated Gibraltar at least 700 feet.

*British Fossil Crustacea.*—Mr. H. Woodward, we are glad to see, continues his account of British Fossil Crustacea. In his fourth Report to the British Association, he described, among other Crustacea, two species belonging to the genus *Cyclus*, one of which still remains unique. Since that date, he has, by the kindness of Professor T. Rupert Jones, F.G.S., received a number of specimens of this genus, collected by Professor Harkness, F.R.S.; Mr. Joseph Wright, of Belfast; and Mr. J. H. Burrow, of Settle, Yorkshire; from the carboniferous limestone, which he describes partly in the last number of the "Geological Magazine" and figures in an interesting plate accompanying his paper.

*The Motion of a Glacier.*—This important question lies for discussion between Canon Moseley and Mr. Croll, F.R.S.E. Mr. Croll has communicated some remarks on the subject to the "Philosophical Magazine" for September. To him there seems to be but one explanation of glacier movement, namely, that the motion of the glacier is *molecular*. His concluding remarks are as follows:—The ice descends molecule by molecule. The ice of a glacier is in the hard, crystalline state, but it does not descend in this state. Gravitation is a constantly acting force; if a particle of the ice lose its shearing-force, though but for the moment, it will descend by its weight alone. But a particle of the ice will lose its shearing-force for a moment if the particle loses its crystalline state for the moment. The passage of heat through ice, whether by conduction or by radiation, in all probability is a molecular process—that is, the form of energy termed heat is transmitted from molecule to molecule of the ice. But at the moment that it is in possession of the passing energy, is the molecule in the crystalline or icy state? If we assume that it is not, but that in becoming possessed of the energy it loses its crystalline form, and for the moment becomes water, all our difficulties regarding the cause of the motion of glaciers are removed. We know that the ice of a glacier, in the mass, cannot become possessed of energy in the form of heat without becoming fluid; may not the same thing hold true of the ice-particle?

*Sir R. Murchison's Report on the Geological Survey and the Museum of Practical Geology.*—This has been published, and will interest the geologist who is desirous of knowing what work the year 1869 has shown in these respects. A total area 2,634¾ square miles was surveyed during the year. This was in England. In Scotland, Mr. A. Geikie reports 111 square miles as being shown. In Ireland 797 square miles have been surveyed. In regard to the museum Professor Huxley reports several valuable additions which have been made during the year. The maps, mining office, and laboratory, are also most favourably reported upon.

*Mr. Forbes' Lecture on Volcanoes.*—In the "Geological Magazine" for November, Mr. D. Forbes, F.R.S., replies to the remarks of Mr. Poulett

Scrope who, in an earlier number commented on his (Mr. Forbes') lecture on volcanoes. Mr. Scrope for the most part agrees with Mr. Forbes, so the difference is not great. But on the subject of the formation of volcanic sand, of the effect of water on molten lava, and some other points, Mr. Forbes here justifies himself fully.

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## MECHANICAL SCIENCE.

*Pneumatic Transmission.*—A very interesting theoretical paper, on systems of pneumatic transmission for passengers and parcels, was read by Mr. Robert Sabine at the last meeting of the British Association. The results of Mr. Sabine's examination of the conditions of working of such a system may be stated to be:—That small pneumatic tubes may be worked more profitably than large ones. For small letter-carrying tubes, and for somewhat larger tubes for the transmission of metropolitan letters to branch post-offices, he thinks they will undoubtedly work satisfactorily. Large tubes of moderate length—for instance for the transport of light goods between different parts of a factory—might be found useful. But he does not believe that a pneumatic line working through a long tunnel could, for passenger traffic, compete, in point of economy, with locomotive railways. In a pneumatic tunnel such as that proposed between England and France, he estimates that in moving a goods train of 250 tons, at twenty-five miles per hour, the mere friction of the air on the walls of the tunnel would amount to 93 per cent. of the whole resistance, and the work usefully expended in moving the train would be only 5½ per cent. of the whole power employed. To propel such a train engines of 2,000 horse power would have to be employed at each end, even supposing the blowing machinery to act with greater efficiency than has yet been attained. He looks to the completion of the Pneumatic Company's works in London and of the pneumatic passenger railway in New York to settle the question of the availability of the system for such purposes. The paper will be found in *Engineering* for September 23.

*The Stability of Ships.*—The controversy which has followed the catastrophe to the *Captain* has led to a re-examination of the stability of some of the ships in the Navy. The *Monarch* has proved to have ample stability for safety, even at light draught. But when quite light it will contribute to her comfort and steadiness, in Mr. Reed's opinion, to admit water ballast into a few of her lower compartments. Provision for effecting this was made in her construction. In the four ships of the *Vanguard* class, Mr. Reed believes it will be advantageous to supply them with a certain amount of permanent ballast, and for that purpose a layer of iron concrete on the bottom is proposed. The need of this ballast arises from the fact that, through improvements in the construction, the hulls of these ships are really lighter than was anticipated, and they float some inches higher than they would have done if these improvements had not been accomplished. Thus the expensive wrought-iron work which on the ordinary system of construction would have been distributed through the hull, will be replaced

by a small amount of cheap concrete placed exactly where it will be most effective in steadying the ship.

*Wrought-Iron and Steel Guns.*—It is stated that in a recent competitive trial between an Armstrong wrought-iron and a Krupp steel gun, the latter has proved to have the greater endurance. After 121 rounds the Armstrong gun split, and was so severely damaged as to be unfit for further service. The steel gun remains in good condition after 210 rounds.

*Indian Railway Gauge.*—A controversy is going on as to the gauge to be adopted for the extension of the Indian railway system. A joint report by Messrs. Strachey, Dickens, and Rendel, and a separate report by Mr. Fowler, have been presented to the Indian Government. In the former a gauge of 2 feet 9 inches is recommended, in the latter a gauge of 3 feet 6 inches.

*Indian Civil Engineering College.*—The difficulty which has attended the system of obtaining young civil engineers for service in the Public Works Department, by competitive examination, has led the Indian Government to establish a college at Cooper's Hill, Surrey, for training its civil engineer officers. Candidates will be admitted to the college by examination. The college course is to extend over three years, with the exception that candidates having more than usual proficiency may shorten the period. Two terms at least of the third year will be passed by the student under a civil or mechanical engineer. It is to be hoped that this attempt at a thorough and systematic system of technical education for engineers may prove a great success. The old apprenticeship system practised in this country, without the supplement of systematic theoretical instruction, does not correspond to the needs of the time.

*Technical Education on the Continent.*—The Institute of Civil Engineers have published a remarkable pamphlet, containing the result of an inquiry into the condition of engineering education abroad.

*Arches of Timber and Iron.*—A very interesting paper on this subject by a foreign engineer, M. Gaudard, has been read before the Institute of Civil Engineers. M. Gaudard explains, amongst others, M. Bresse's method of estimating the stress in iron arches, and a method of M. Durand Claye of comparing the boldness of different arches, which will be new to most English readers.

*Metaline Bearings.*—Some new materials for forming the bearings of machinery have been introduced by Dr. Gwynn of New York under this name, the idea being to obtain sufficient solidity in the material for the bearing to retain its form, and sufficient plasticity to reduce the friction. For example, very fine powder of iron and of tin are intimately mingled by grinding, and then compressed by hydraulic pressure; or solder is reduced to a semi-fluid condition by heat and mixed with powdered graphite, and then consolidated by pressure.

*Wrought-Iron Bridges.*—We may direct attention to a remarkably interesting appendix to the last volume of the Transactions of the Institute of Civil Engineers, consisting of a paper by Mr. Calcott Reilly on wrought-iron bridges. Mr. Reilly gives in detail all the calculations made during the actual process of designing two well-considered iron bridges. It is not often that the actual process of designing is so fully laid bare by a very

competent engineer for the instruction of others; and in regard to riveted joints, which are often very carelessly arranged, Mr. Reilly proceeds on principles not only original and sound, but in advance of current practice.

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## MEDICAL.

*The Sphygmograph invented by Galileo.*—It would seem from a letter of Mr. Charles Williams, in the "Lancet" of November 26, that Galileo really devised an apparatus for estimating the velocity of the pulse. Whether his instrument and the present one are the same is a question, but there can be no doubt that a sphygmograph of some kind or other was devised by Galileo.

*Photography in Medicine.*—From the same number of the "Lancet" we learn of the establishment in America of a "Photographic Review of Medicine and Surgery." It is to be published once every second month. It contains four photographic plates.

*Substitution of Salts in the Bones.*—The "Lancet" of December 1 records some experiments that have been made in the course of the past year by M. Papillon, and which are recorded in the "Comptes Rendus." In one of these a young pigeon was dieted on distilled water to which hydrochlorate, carbonate, sulphate, and nitrate of potash were added, and with grain made into a paste with strontia. The bird remained in perfect health for nearly eight months, when it was killed, and an analysis made of its bones, with the following results: in 100 parts there were of lime 46·75, of strontia 8·45, of phosphoric acid 41·80, and of phosphate of magnesia 1·80; residue, 1·10. In a second experiment a white rat ten days old was subjected to a similar regimen, except that phosphate of alumina was substituted for the strontia given to the pigeon in the proportion of about a grain and a half per diem. The animal remained to all appearance in good health for about six weeks, when it died suddenly in convulsions. An autopsy showed the presence of intense enteritis. Analysis of the bones showed that in 100 parts there were of alum 6·95, and of lime 41·10 parts. Another animal of the same litter was supplied with phosphate of magnesia instead of phosphate of alumina, and was killed at the same time. Analysis of its bones showed the presence of magnesia in the following proportions in 100 parts: magnesia 3·56, lime 46·15. In all the animals the appearance presented by the bones was natural, and they seemed to possess their ordinary physiological peculiarities.

*Detecting the Blood of Animals.*—It would seem that the questionable discovery of Herr Neumann has received confirmation by Dr. Day, of Geelong Institute, viz. that the picture or net-work formed by human blood can be distinguished under the microscope from that which is formed by the blood of other animals. He says he has repeated the experiment, which is "wonderfully simple," almost every day for the last two months, with invariable success. A small drop, not a mere speck, of the blood is to be placed on a microscope-slide, and carefully watched, at a temperature of 10° or 12° Réaumur (=54·2° to 59° Fahr.), until the picture or net-work formed

by its coagulation is developed. Human blood speedily breaks up into a "small-pattern" net-work; the blood of other animals (calves, pigs, etc.) takes a longer time, and makes a large pattern; but the blood of every animal seems to form a characteristic "picture." Dr. Day has examined the blood of calves, pigs, sheep, rabbits, ducks, hens, several kinds of fishes, etc., as well as that of man, and has found the results to be trustworthy and constant.

*A Medical College for Women* has, according to the "New York Medical Journal" for November, been organised in Chicago, Ill., with a faculty of no less than fifteen professors. We pity the girls if they are expected to undergo the infliction of a full course of lectures from each of the fifteen. Our Chicago brethren must have singular ideas about the meaning of titles, for one of the incumbents is announced as "*Professor Emeritus*"—and this in an institution that as yet has no existence.

*Food for Troops.*—Dr. Dingler gives an account of a new Prussian method. It appears that a Berlin cook, named Grunberg, has recently discovered a process by which a preparation of peas may be made so as to keep without becoming sour, and the Prussian Government has bought the secret of this process from the inventor, for a sum of 5,555*l*. The Prussian War Office has created an establishment, at Berlin, capable of producing daily 75,000 sausages made of this preparation, which consists of a mixture of bacon, peculiarly prepared pea flour, onions, and other ingredients, inclusive of salt. The sausages are sent away packed in boxes containing from 100 to 600, weighing 1 lb. each, which are destined as food for the armies, and only requiring to be boiled in water for a very short time to be ready for the use of the soldiers. The daily ration of each is 1 lb., a quantity quite sufficient for each man. This establishment, only working for the armies, costs daily about 6,000*l*.

*Relations of Consciousness and Seat of Sensation.*—A very able paper is pubished on this subject in the November number of the "Journal of Anatomy" by Dr. Cleland, of Queen's College, Galway. We can only give the following abstract, and hope the subject will be intelligible to our medical readers. (1) The irritation of a nerve of common sensation throws the nerve into the impressed condition, and as soon as that condition is continued to the brain, the mind recognises the irritation at the site where it is applied, in the form of sense of touch, temperature, or pain, according to its character. Over-intensity of the impressed condition may also itself be recognised in the form of pain. (2) Nerves of special sense differ from those of common sensation both in the circumstance that the apparatus at their extremities is affected by irritations of a different kind from those which affect other nerves, and in their irritation being recognised in the form of the special sense to which they are devoted. (3) By the impressed condition continued from the brain to the distribution of a motor nerve not only is a stimulus communicated to the muscles and applied to the nerve, but muscular sense is given; and, the consciousness being brought into direct communication with the part, the will is enabled to regulate the position of the part and the degree of muscular energy with which it is maintained. But a motor nerve differs from sensory nerves of all sorts in the .act that irritation of it does not produce any sensation either of the character of common sensation

or special sense; and in this respect it is like the proper fibres of the spinal cord and brain.

*A Woman with Four Breasts.*—A primiparous woman was admitted under M. Lorain, and was delivered next day of a dead premature child. She was found to have four breasts, two in the normal position and with the normal puerperal appearances, and two which, from their position, might be called axillary, and attaining the size of a small orange.—*Revue photographique des Hôpitaux.*

*Permanganate of Potass in Coryza.*—We desire to call attention to the use of permanganate of potassa in very dilute solution (0·18 grms.=1·07 grains to 60 grms. or c.c.=to about 2 fluid ounces) against coryza, cold in the head, attended with severe sneezing. Of the permanganate solution, some twenty to sixty drops are poured into a tumblerful of water, and of this liquid every two hours a quantity of a tablespoonful is snuffed up the nostrils; and if there be any soreness of the throat, the same liquid is applied as a gargle. Dr. Franck, of Münich, states that he has prescribed this mode of treatment now for some years, and found it very efficient, by curing the complaint in about from two to four days.

*Skin Grafting.*—This remedy now appears quite successful. The "Medical Press" of November 30 says that Mr. Pollock exhibited to the Clinical Society several cases of the operation devised by M. Riverdin, of Paris, in 1869. A girl, aged eight, had been in St. George's Hospital with an ulcerated surface from buttock to knee, which had existed for two years. Mr. Pollock first transplanted two small pieces of skin, about the size of millet seeds, taken from the lower part of the abdomen. Fourteen pieces in all were transplanted at various periods. The burn was nearly healed in five months, without any perceptible contraction of the cicatrix. Mr. Pollock transplants usually very small pieces, and takes care that the granulations are healthy where he inserts them. Mr. Lawson showed a patient in whom a large ulcer in the leg had resisted all treatment for four years, and was completely cured in four weeks after a piece of skin, the size of a fourpenny-piece, had been planted on it. As soon as the new skin had established its vitality, granulations sprang from the circumference, and rapidly closed in the wound. The granulations should be healthy, no fat transplanted, but only skin, which must be accurately applied to the granulating surface. The new skin is kept in position without interruption, and lightly covered with a layer of lint, over which is a small compress of cotton wool and a bandage, for the purpose of keeping it warm until it grows on to the part.

*The Indian Medical Service.*—We regret to state that there will be no February examination this year.

*Urea formed in the Liver.*—According to a note in the "New York Medical Journal" for November, the latest researches upon the place of origin of urea, and especially the beautiful experiments of M. Gréhant, have demonstrated that the kidneys are by no means secretory, but purely excretory, organs for urea. Dr. Cyon, in the last number of the "Centralblatt," publishes a few facts in the form of a provisional communication, to show that it is probably produced at the liver. The plan of experimentation adopted (in common with M. Istomin) was as follows: The whole of the blood was abstracted from the carotid of a dog, and a portion, after being

defibrinated, was transmitted by means of mercurial pressure through the liver. Coincidently three canulæ were introduced—one into the inferior vena cava, the second into the hepatic artery, and the third into the vena porta. The results of careful analysis showed that the blood which had passed through the liver contained a much larger proportion of urea than ordinary arterial blood. In one experiment 100 c. c. of the arterial blood when defibrinated contained 0·08 grammes of urea; but, after having been passed four times through the liver, the same quantity contained 0·176 grammes.

*Destruction of Tumours by Injecting Chromic Acid.*—Chromic acid has not been much employed here. In America, however, it has been used with advantage. In a late number of the "Philadelphia Medical and Surgical Reporter," Dr. Daniel Leasure, of Alleghany City, gives an account of a tumour of the neck (probably malignant) treated by the injection of chromic acid. It was situated on the right side, one inch and a half by two inches and a half longest diameter. On September 17, 1869, it was injected by a hypodermic syringe with sixty drops of a solution of chromic acid, 100 grains to the ounce of water. On the day following, and on the third day, it was injected as before. No serious irritation. On Sept. 30 repeated, the tumour softening. Oct. 17, the same. Nov. 15, the tumour had collapsed. An opening formed, and matter was discharged. Poulticed, and on Nov. 29, reported well. There was a small cicatrix, three lines in diameter, at the seat of the late opening, which so closely resembled in colour the surrounding skin as to be scarcely noticed. In June 1870 no sign of return. Two other similar cases treated with like success. The use of chromic acid in this manner is new.

*Poisonous Snuff.*—We understand that Dr. Garrod lately lectured at King's College on a case of lead-poisoning, in which the mineral was taken in snuff. It was rappee that the patient habitually took, and the damp snuff, packed in the usual lead cases, converted some into carbonate. The symptoms were serious, and with difficulty traced to their real source. Then several packages were purchased, and found to be contaminated with the poison.

*The Late Dr. Robert Knox.*—We learn, from the "Medical Press" of Dec. 7, that Dr. Lonsdale has just brought out a life of Robert Knox, the once famous lecturer on anatomy, of Edinburgh. From this interesting biography it seems that the terrible affair of Burke and Hare in 1828 proved the ruin of this distinguished teacher's fame in Edinburgh, and that he died comparatively unknown and poor in Hackney parish, London. One of his last works, which has been much admired by some, is the "Races of Man."

*The Conducting Power of the Nerves.*—In the "Lancet," Dec. 17, are recorded some fresh investigations upon this point, by MM. Place and Helmholtz. M. Place adopted the same method as that formerly suggested by Helmholtz. The cylinder employed for registration was that constructed by Heynsius for the Leyden physiological laboratory, and was carefully planted in such a manner as to avoid accidental vibration. The measurement of the time was estimated by a coincident tracing from a vibrating tuning-fork. The median nerve was irritated first where it runs in the

bicipital groove, and afterwards at the wrist, where it runs on the ulnar side of the musculus flexor carpi radialis, the distance of the electrodes being about 300 millimètres, or above one foot. The muscular contractions produced were strong, a more powerful current being required for the excitation of the nerve above where it lies deepest. In order to obtain trustworthy results, one or two minutes were allowed to pass between each experiment. The best observations gave a result of from 50 to 60 mètres per second as the rapidity of propagation of motor impulses, with a mean of 53 mètres per second. This differs considerably from the estimation of Helmholtz, who estimates it at only 33 mètres per second. The point of excitation in Helmholtz's experiments was somewhat higher than in those of M. Place; and when Helmholtz's point was taken, a number (35·25 mètres per second) not very different from his was obtained. Further investigations showed the general truth of Munk's observation, that the rapidity of propagation of impulses was much greater in the peripheral than in the central portions of a nerve. Helmholtz's observations were carried on with a new instrument, of which the idea was suggested by Fick. He found that variations in temperature exert an important influence on the rapidity of propagation of motor impulses, the rapidity being much greater in warm than in cold temperatures. He found also that when two induction-currents are passed through, an interval of at least 1–500th of a second must elapse between them in order that the second stroke should produce an augmentation of the muscular contraction. If the period be less than this, the two act as a single shock; with an interval of 1-300th of a second the augmentation is very perceptible. Constant currents readily produce tetanus, especially when passed in a downward direction. Oscillations are felt in the muscle, the duration of which amounts to 0·09 of a second.

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## METALLURGY, MINERALOGY, AND MINING.

"*Mineralogical Notices*" was the title of a paper read before the Royal Society on November 17, by Professor Maskeyne and Dr. Flight. It deals with several minerals, and we may mention a few of them. The first is "On the Formation of Basic Cupric Sulphate." In 1867 M. Pisani described a mineral which he supposed to be the Woodwardite of Mr. Church. The substance, however, is not the latter mineral. It had previously been examined in the laboratory of the British Museum, and the results sufficiently tallied with those of M. Pisani to identify the mineral. It can be divided into an inner layer and an outer crust, of which the contents were given. 2. An opal from the Waddela Plain, Abyssinia. Mr. Markham presented to the British Museum some remarkable specimens of green opal from the above locality. An analysis is given of it. Next follow *Frankolite* from Cornwall; *Epidote* and *Serpentine* from Iona; *Cronstedtite*, *Pholerite*, and others. The paper is reported in the "Chemical News" of November 25.

*Steel Rails.*—There are in the United States, says the "Philadelphia Ledger," 46,000 miles of railway which it is necessary to re-lay with steel rails. It takes 100 tons of rail to lay a mile of road. The estimation of

200,000 tons would only re-lay 200 miles annually. If steel rails were admitted free, their consumption in America would be enormous, and the advantage to railway companies correspondingly great.

*The Chemical Nature of Cast Iron.*—The committee appointed in 1869 have reported that they have made very little progress. They promise a good report for the next year. Those on the committee were Mr. David Forbes and Messrs. Abel and Matthiessen. The latter is unfortunately dead since.

*South African Diamonds* continue to arrive, and some of them are extremely fine. The subject was discussed in a paper read by Professor Tennant before the Society of Arts on November 23. The paper appears with a woodcut in the "Journal" for the 25th, and may be of interest to some readers.

*Ozokerit.*—The mineral ozokerit, says a recent number of the "Medical Press," the celebrity of which has been achieved, it is said, by an expenditure in advertising of something approaching 15,000*l.* by Messrs. Field, of London, was exhibited by Mr. Henry Draper, at the last meeting of the Dublin Chemical Club. This substance is found overlying the coal measures in Moldavia, Austria, and at the Urpeth Colliery at Newcastle-on-Tyne. It is purified by distillation, and afterwards by pressing it and treating it with sulphuric acid; when purified it has an extremely high melting point. For this characteristic, which has not been found in any other similar substances except wax, it is selected for the manufacture of candles, because it affords a larger wick and a better illuminating power at a lower price. From Mr. Draper's experiments it appears that pure ozokerit and white wax melt at 150° F., the candle as sold at 138°, paraffin at 129°. There is, therefore, an admixture with it of some other material. Even the residue of ozokerit, after its purification, has been utilised. The late Dr. Matthiessen, whose untimely death by his own hand is still in our memory, patented it as an insulator for telegraphic wires, for which purpose it is said to be eminently suitable.

*Enamelling of Iron.*—This important subject is discussed by Herr Dr. E. M. Dingler, in the Oct. (No. 2) number of "Dingler's Journal," and the paper is well reported in the "Chemical News." The author first points out that the main difficulty in the process of enamelling iron consists in the fact that the expansion of the metal by any increase of temperature is far greater than the expansion of glassy bodies, and that, as a consequence, the enamel is very liable to come off by a sudden increase of the temperature of the enamelled metal. This defect, says the author, is best mended by the use and application of a ground or first layer, which does not become quite fluid by heat, but retains a pasty consistency and porosity, which enable it to give way to some extent by the expansion. On this first layer, the second or covering layer is applied. The author recommends the following mixture as ground or first layer:—Pulverised quartz, 30 parts; borax, 16½; white-lead, 3 parts. These ingredients are fused together, and the molten mass is pulverised and intimately mixed with 9 parts of very finely ground-up quartz, 8⅔ parts of washed pipeclay, and ½ part of magnesia alba. The second, or covering layer, is prepared by melting together a mixture of 37½ parts of pulverised quartz, 27½ of borax, 30 of oxide of tin,

15 of carbonate of soda, 10 of nitrate of potassa, and 5 parts of magnesia alba. The molten mass is poured into water, and afterwards mixed with $6\frac{1}{8}$ parts of powdered quartz, $3\frac{2}{3}$ of oxide of tin, $\frac{2}{3}$ of carbonate of soda, and $\frac{3}{4}$ parts of magnesia alba, and the whole of the ingredients very finely ground along with water, so as to constitute an inpalpable powder. The metal to be enamelled is first cleaned with dilute sulphuric acid (1 part of strong acid to 24 of water), then rubbed with sharp sand, rinsed in hot water, and immediately afterwards dried; the enamel masses are next burnt on. Well-made enamel exhibits the following characteristics:—A perfectly smooth and level surface, so that the surface nowhere feels uneven or rough to the touch; a pure white colour; absence of small cracks.

*Copper and Manganese Alloys.*—Mr. J. F. Allen, F.C.S., has made several interesting alloys of the above. These are, shortly, 1st, Manganese and copper in various proportions, from 35 per cent. to 5 per cent. of iron, as ingot, sheet, or wire; 2nd, Copper, zinc, and manganese, also in different proportions, and in a variety of applications; 3rd, Copper, zinc, manganese, and tin as ingots and as bearing; 4th, Copper, manganese, and tin in several different proportions as bars; 5th, Copper, manganese, and lead.

*The Metals in the Sun. Reversal of Sodium Lines.*—Mr. C. A. Young writes a letter to Professor H. Morton (published in the "Chemical News," Oct. 21) regarding some recent spectroscopic observations in the sun. The following is the letter:—"Dartmouth College, Hanover, N. H.; Sept. 26, 1870.—My dear sir,—I write to inform you that last Saturday, Sept. 22, about 11 A.M. Hanover mean time, I was so fortunate as to see the sodium lines $D_1$ and $D_2$ reversed in the spectrum of the umbra of a large spot near the eastern limb of the sun. At the same time C and F lines were also reversed, but with the great dispersive power of my new spectroscope I see this so often in the solar spots that it has ceased to be remarkable. In the umbra of the spot the $D_3$ line was not visible, but in the penumbra was plainly seen, as a dark shade. I am not aware that this reversal of the sodium lines in a spot-spectrum has ever been observed before; its reversal in the spectra of prominences is not very unusual. A small prominence on the western limb of the sun, which was visible the same forenoon, presented all the following bright lines, viz. C, $D_1$, $D_2$, $D_3$, 1474·, $b_1$, $b_2$, $b_4$, 1989·5, 2001·5, 2031·, F, 2581·5, 2796·, and $h$; fifteen in all. In the spot-spectrum the magnesium lines $b_1$, $b_2$, and $b_4$, were not reversed, but while the shade which accompanies the lines was perceptibly widened, the central black line itself was thinned and lightened."

*What is Wocheinite?*—Dr. H. Schwarz (in "Dingler's Journal") gives an account of some experiments made with a hydrate of alumina found in the Wochein, and akin to the Bauxite. This wocheinite consists, in 100 parts, of—alumina, 56·82; silica, 11·28; peroxide of iron, 1·60; water, 24·20; traces of carbonate of lime and of manganese. The mineral is suitable for being mixed with other fire-clays (it is not sufficiently plastic by itself, even when pulverised), and for the manufacture of aluminate of soda and other salts of alumina.

*Composition of Columbite, Ferro-ilmenite, and Samarskite.*—In the "Journal für prakt. Chemie," No. 13, Herr A. Hermann states the

analysis of the above three minerals. Beyond this the paper has little interest.

*A Concrete which has several good Properties.*—The Rev. H. Highton, M.A., has described to the British Association a form of concrete which appears inexpensive and useful. The following is the method of preparing it:—A concrete is made with any good hydraulic cement. When this is dry it is steeped in an alkaline solution of silica, in which is placed a quantity of free silica. The following chemical process then takes place: the lime in the concrete extracts the silica from the solution, leaving the alkali free, which immediately attacks the free silica and conveys it in its turn to the concrete. This process goes on continually till the lime in the concrete is saturated with silica. By this process within a week the strength of the concrete is increased from 50 to 150 per cent., and by a longer continuation of the steeping the strength is still more increased. As the alkali acts only as a carrier of the silica, it is used over and over again; and it is in this that the economy of the manufacture consists. The following is the comparative resistance to a crushing force of several kinds of stone:—

| | Per sq. inch. lbs. |
|---|---|
| The Silicated Concrete, or Patent Victoria Stone | 6,441 |
| Aberdeen Granite | 7,770 |
| Dartmoor Granite | 6,993 |
| Peterhead Granite | 6,216 |
| Yorkshire Landing | 5,851 |
| Stafford Blue Brick | 4,032 |
| Portland Stone | 2,426 |
| Bath Stone | 1,244 |

The stone formed in this manner has been tried as a pavement in the busiest part of Cheapside, and in many other parts of London, and for steps, lintels, sills, &c., in many parts both of this kingdom and abroad, as well as in India. The whole of the stone in the new warehouses, 27 St. Mary Axe, is made in this manner.

*Alloy of Lead and Platinum.*—The "Chemical News" gives the following account of the above alloy, prepared by Herr A. Bauer, which originally appeared in the "Berichte der Deutschen Chemischen Gesellschaft zu Berlin," No. 15, 1860. After referring to the observation of M. Deville, that an alloy of lead and platinum is readily decomposed in consequence of the conversion of the lead into white-lead, the author made an alloy, consisting of 3 parts of pure lead and 1 part of platinum. This alloy is so brittle that it can be readily pulverised; and the powder so obtained was moistened with water, and placed under a bell-jar exposed to the action of carbonic acid, oxygen, and vapours of acetic acid. The conversion of the lead into white-lead took place rapidly; and after it appeared that all the lead was converted into white-lead, the powder was treated with acetic acid, and the residue again exposed under the bell-jar to the action of the same substances. This process having been repeated several times, there remained at last a steel-greyish coloured crystalline powder, which only appeared to be finely divided platinum. On being treated, however, with dilute nitric acid, the author found that the powder consisted of an alloy of lead and

platinum, which contained, in 100 parts—Platinum, 48·82; lead, 51·18; corresponding to the formula Pt + Pb. This alloy has a specific gravity of 15·77, is readily decomposed by mineral acids, but withstands boiling with acetic acid.

## MICROSCOPY.

*Death of the President of the Royal Microscopical Society.*—On December 12 the Rev. J. Bancroft Reade, M.A., F.R.S., F.R.A.S., passed away from among us, at the mature age of seventy years. He might have lived much longer had it not been for the presence of a peculiar cancerous affection of the rectum, though, singularly enough, it was really a liver affection which removed him from among us. He was a dear old man, and there must have been few who knew him who did not also love him. He was a man not as well known as he deserved to be, for during the last forty years he has been actively engaged in discoveries, both connected with the microscope and photography. A full sketch of his life will, we believe, appear in an early number of the "Monthly Microscopical Journal." Meanwhile a very good sketch of his photographic labours will be found in the "British Journal of Photography" of December 16.

*The Condition of the Microscope.*—The question as to whether the object-glasses are perfect, or are in a condition very far from perfection, still goes on; Mr. Wenham approaching the former side, Dr. Royston-Pigott the latter. The controversy is not yet done, and in the present number of the "Monthly Microscopical Journal," we believe, a paper appears from Mr. Wenham, re-asserting his conclusion. As yet it is difficult to decide who is right. Time will tell.

*Mounting Diatoms.*—This, it must be confessed, is seldom done rightly. In the "Monthly Microscopical Journal" for December a paper by Captain Lang gives some useful hints. He says that of course all diatoms should be mounted on the cover. To secure the correct centering of them, he forms on a glass slip, by means of the turntable, a ring of gold-size ⅝ths of an inch in diameter, the size of his covering glass, and within this a very minute one exactly in the centre. This is hardened by heat, as his cells are. On the outer ring, at equal distances, are placed three little bits of beeswax. The covering glass, on which it is intended to arrange the diatoms, is placed on this general mounting slip, and slightly pressed on the wax. Instead of distilled water, he places on the cover a very slight smear of glycerine, into which previously, as in the case of the water, a drop of gum may have been added. Into this the diatoms are dropped, and may then be pushed within the inner ring, and their perfect arrangement and centering are secured. The advantage of the glycerine over the water is that it is a greater solvent for freeing the diatoms from any extraneous dirt, and that it will remain moist for any length of time. When the arrangement is completed, the covering glass is gently pushed off the three pieces of wax, and transferred from the slide to the hot plate, when in a few minutes the glycerine is evaporated. Put on another slide under the microscope, a drop of benzole is placed on the diatoms, and whilst they are being

permeated by it, and all air displaced, a little balsam is dropped into the prepared cell, when the cover is seized by the forceps, reversed, and placed carefully on it, and the mounting is completed in less time than it has taken to tell the process.

*The New Sponges.*—Mr. Kent has contributed some interesting papers on the above to the "Monthly Microscopical Journal." Readers of these will find Dr. Oscar Schmidt's new work full of interesting details anent the sponges found in his recent dredgings. Some of Schmidt's drawings are indeed beautiful. The work is almost folio in point of size, and is illustrated by six plates, which are unquestionably superior to almost anything done in this country. The memoir should be carefully read by those who are interested in the subject.

*Passage of white Corpuscles through the Walls of Vessels.*—This question would not appear to be decided yet, though there is much evidence in favour of their creeping out. From the details of a paper by Col. Dr. Woodward, in the "Monthly Microscopical Journal" for October, it would seem that, so far as the structure of the vascular walls and the passage of the white corpuscles through them are concerned, the facts appear to be on the side of Cohnheim. How, then, with regard to the doctrine of inflammation which he builds upon these facts and upon his corneal studies? Does the creeping out of the white corpuscles constitute the essence of the inflammatory process? Do these little movable masses of living protoplasm furnish the germs for the elements of new formations? Have pus-corpuscles no other origin? Are the processes which go on in the cells of the inflamed tissue purely passive—mere phenomena of retrograde metamorphosis?

*Dr. Carpenter's Last Voyage.*—Dr. Carpenter has laid the results of his last voyage ("Malta, the Mediterranean, &c.") before the Royal Society. The paper read was full of interesting details. It is not yet published, and when it is we shall of course lay it before our readers. Several new species have been found, and some curious facts about the distribution of animal life have been discovered. The copy of the proceedings containing the Professor's former paper extends over one hundred pages. The first part deals with the apparatus, and it shows us that the author has paid attention to even the most adverse criticism of his earlier voyage, and has on this occasion taken care to have all the apparatus constructed upon the most thoroughly scientific principles. It is greatly to be regretted, though, that the electric sounding apparatus was found not to work sufficiently well at sea to enable Dr. Carpenter and his assistants to employ it. The details are singularly minute, and the appended reports by W. Lant Carpenter, B.A., B.Sc., Dr. Frankland, F.R.S., and Mr. David Forbes, F.R.S., though short, are of an excellent character.

*Pritchard's Infusoria.*—A new edition is coming out, greatly improved. At a late meeting of the New York Lyceum of Natural History, Professor T. Egleston, jun., read a letter received from Dr. Eulenstein, of Berlin, speaking of his forthcoming revised edition of "Pritchard's Infusoria," and asking for contributions of specimens for the purpose of furthering that undertaking.

*Browning's Spectroscope Tables.*—These are excellent, and produced at a

very low price. Each table is about the size of this journal, and contains seven or eight spectra, with the principal bands marked out.

*Progress of Microscopical Science.*—The work done during the past quarter has been valuable, as will be seen from the subjacent list of the articles contributed to the three numbers (October, November, and December) of the "Monthly Microscopical Journal." "The Patterns of Artificial Diatoms." By Henry J. Slack, F.G.S., Sec. R.M.S.—"On Ancient Water-fleas of the Ostracodous and Phyllopodous Tribes (Bivalved Entomostraca)." By Professor T. Rupert Jones, F.G.S.—"On the Real Nature of Disease Germs." By Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians, and Physician to King's College Hospital.—"On the Histology of Minute Blood-vessels." By Brevet Lieut.-Col. Woodward, Assist.-Surgeon U.S. Army.—"On the Formation of Microscopic Crystals in Closed Cells." By A. W. Wills.—"The Ciliary Muscle and Crystalline Lens in Man." By J. W. Hulke, F.R.C.S., F.R.S. Part II.—"On the 'Hexactinellidæ,' or Hexradiate Spiculed Silicious Sponges taken in the 'Norna' Expedition off the Coast of Spain and Portugal. With Description of New Species, and Revision of the Order." By W. Saville Kent, F.Z.S., F.R.M.S., of the Geological Department, British Museum.—"On a Mode of Ascertaining the Structure of the Scales of Thysanuradeæ." By Joseph Beck, F.R.M.S., F.R.A.S.—"On the Advancing Aplanatic Power of the Microscope, and New Double-Star and Image Tests." By G. W. Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., &c. &c.—"A Few Remarks on Dr. Bastian's Papers on Spontaneous Generation." By Metcalfe Johnson, M.R.C.S.E., Lancaster.—"American Microscopes and their Merits." By Charles Stodder.—"On a New Anchoring Sponge, 'Dorvillia Agariciformis.'" By W. Saville Kent, F.Z.S., F.R.M.S., of the Geological Department, British Museum.—"On Aplanatic Definition and Illumination, with Optical Illustrations." By G. Royston-Pigott, M.D., M.A., &c.—"On Selecting and Mounting Diatoms." By Captain Fred. H. Lang, President of the Reading Microscopical Society.—"On Certain Cattle Plague Organisms." By Boyd Moss, F.R.C.S.—"Notes on New Infusoria." By J. G. Tatem.

## PHYSICS.

*The Royal Society.*—The annual meeting of the Fellows of this society was held on November 30, at Burlington House. The President, Lieut.-General Sir Edward Sabine, K.C.B., &c., delivered the inaugural address, in which he reviewed the progress which had been made in science during the year. In closing his address Sir E. Sabine announced his intention not to offer himself for re-election at the next anniversary, when, to quote his words, he "will deliver over the chair, doubtless to a younger, it may well be to a worthier, occupant; it can hardly be to one having the welfare of the Royal Society more warmly at heart"—a sentiment which was cordially echoed by all present. The presentation of the medals followed:—The Copley Medal was awarded to Mr. James Prescott Joule, F.R.S., for his experimental researches on the dynamical theory of heat; a Royal

**Medal to Professor William Hallowes Miller, Foreign Secretary R.S., for his researches and writings on mineralogy and crystallography, and his scientific labours in the restoration of the national standard of weight; a Royal Medal was also awarded to Mr. Thomas Davidson, F.R.S., for his works on the recent and fossil Brachiopoda, more especially his series of monographs in the publications of the Palæontographical Society; the Rumford Medal to M. Alfred Olivier Des Cloizeaux, for his researches in mineralogical optics.**

*Tool Paring and Cutting.*—At the meeting of the Literary and Philosophical Society of Manchester on November 15, Mr. Johnson, in an interesting paper, pointed out the great advances that have been made in this department of late. He finally showed to the meeting some specimens of steel and iron parings sent to him by Messrs. Smith and Coventry, machinists, Salford, and further remarked that these parings demonstrated very clearly the capabilities of the machines and cutting tools of the present day. One specimen, from a Bessemer steel shaft, the result of taking a cut ⅝ths of an inch deep by ⅝ths of an inch traverse, was particularly interesting on account of the form and size in which they, the parings, left the cutting tools. The cutting tools used in obtaining the specimens exhibited to the meeting were of a peculiar construction, and possessed some marked advantages over those in ordinary use.

*Musical Pitch.*—The conductors of the "Society of Arts Journal" have adopted an excellent plan. They have written to the musical department of nearly every European State, making enquiries as to the "pitch" adopted therein. The letters returned have been most valuable; and to render them still more so, their contents have been arranged under several heads in a scheme of classification, so that by looking over two or three pages every information concerning this point may be gained.

*The Fusibility of Platinum in the Blowpipe.*—Mr. W. Skey, analyst to the Geological Survey of New Zealand, has a short note on this point in the "Chemical News" of Dec. 2. The metal platinum has hitherto been supposed to be infusible, except at a temperature that is so high as to be incapable of being produced by the common blowpipe. When he was lately engaged in studying the effects of the hot-blast blowpipe flame, the results of which investigation have already been communicated to the Wellington Philosophical Society, he found it necessary to test, with accuracy, the degree of fusibility of platina; and discovered that if the loss of heat from the flame, by conduction, was guarded against, platinum can be fused with an ordinary blowpipe blast through a candle flame. The method adopted was to substitute, for the metallic nozzle generally employed, a tube of clay or glass, either of which is a feeble conductor of heat, as compared with metals. By this means fine platinum points were fused in an unmistakable manner to beads. The blast was that ordinarily used in the laboratory by the use of the hydrostatic blowpipe, the flame being that of a stearine candle.

*Levelling and Survey in Switzerland.*—In the "Archives des Sciences physiques" (No. 4) MM. Hirsch and Plantamour give an important paper on this subject. The paper contains an interesting account of the labours of a committee of scientific men and engineers, who, acting upon the suggestion made at an international meeting held at Berlin in 1864, under the presi-

dency of General Baeyer, set to work to execute, in the Helvetian Republic, a complete and very accurate taking of levels and measuring of altitudes above sea-level, and other geodesical labours. The observatory at Berne is situated at 572·14 mètres above sea-level; the cathedral of Fribourg at 588·66 mètres; the town-hall at Chaux-de-Fonds at 989·35 mètres.

*Physics at Cambridge.*—We learn from a contemporary that the difficulty of providing funds for the establishment of a Professorship of Physical Science in the University of Cambridge has been overcome by the colleges, at a meeting of their heads, taking upon themselves a quota of the rates for improvements and other purposes in the town of Cambridge, which was formerly charged upon the University funds. This sum amounts to more than twelve hundred pounds per annum; so that the University will speedily be able to avail itself of the munificent offer of the Duke of Devonshire, and will doubtless proceed at once to establish a Professorship of Physical Science, and obtain the other aids in the way of laboratory, apparatus, and assistants, that the Professor may require.

*Physics at Oxford.*—It is now stated that the Physical Laboratory lately built at Oxford is opened this term for practical instruction in physics, under the superintendence of Professor R. B. Clifton, F.R.S., assisted by two demonstrators.

*The Laws of Distillation.*—It is a fact that volatility alone does not determine which of mixed liquids will distil over first. Quantity has something to do with it. If the less volatile be in large excess, it tends to come over with the other. But there is still another law. The comparative density of the vapours produced affects the result, the denser vapour having a tendency to be evolved in greater quantity. Dr. Van der Weyde thus closes a recent paper:—"These facts prove that the amount of vapour developed from liquids is regulated by volume and not by weight, or, in other words, that of two liquids possessing the same boiling point, but of which the densities of the vapours differ, the same volumes of vapours will evolve, and that, consequently, the liquid emitting the densest vapour will evaporate in larger quantity; or that if there be two liquids of which the boiling points differ, and that with the lowest boiling point possesses the lightest vapour, the greater volume of the vapour generated from the latter will produce less liquid after recondensation than the lesser volume of the vapour evolved from the less volatile liquid, the latter thus more than compensating the former, and resulting in the apparent anomaly that from a mixture of two liquids of different boiling points the least volatile may sometimes distil over in the largest quantity."

*The Laws of Electric Batteries.*—Mr. H. Highton contributes to the "Chemical News," Dec. 2, some novel ideas on the subject of electric batteries. He is evidently working the subject out in its right vein—dividing the time during which the battery acts into three periods. He thus describes them. (1) The time which it takes the electric force to traverse the circuit after it is closed. This in ordinary cases is infinitesimal; but where the circuit is very long, and where there is much inductive as well as conductive resistance, as in the Atlantic telegraph, the time becomes very appreciable. But he would remark that the expenditure of zinc during this first period is occupied in producing a state of tension in the conductor which, theoretically

speaking, forms a store of available force that may afterwards be recovered. (2) There is the period during which the magnet is pulling its keeper to itself, and is positively doing actual work. Now, paradoxical as it may seem, it is a fact that during this period (No. 2), while the magnet is doing actual work, the intensity and the consumption of zinc is actually diminished, so that, in one sense, the more work done the less the fuel (so to speak) which is consumed in doing it. It is in this point that a galvanic battery differs from other machines which do work. It is as if a horse, when he did work, ate actually less food than when he was idle, and wasted less muscle, or as if a locomotive consumed less fuel when in motion than when at rest. (3) Then comes the third period, when the keeper has been pulled home, and the weight is merely sustained. During this period no actual work is being done, and the weight sustained only shows what work the magnet is capable of doing, and so serves as a measure of its potentiality, and during this period (No. 3), strange to say, more zinc is consumed per unit of time than while the work is being done. In practice, therefore, in an electro-dynamic engine, period 3 should be reduced to *nil.* Now break the circuit. If there be a secondary wire (as in a Ruhmkorff coil) the tension produced during the first period may be utilised in some way by the counter current produced in thus breaking the circuit, and the same series of phenomena may then begin over again.

*The Films of Liquids, and Cohesion Figures.*—In connection with this matter, Mr. Charles Tomlinson, F.R.S., has been making some interesting experiments. Under date Nov. 28, he writes an account of the following experiments to the "Chemical News":—A very strong solution of sodic sulphate was boiled in a large flask and then filtered into six small flasks, each of which was again boiled and set aside to cool, covered with a watchglass. Some oil of citronella, diluted with two or three times its volume of ether, was taken up in a straight dropping-tube furnished with an india-rubber shield, as described in my mode of testing M. Jeannel's experiment (see "Chemical News," vol. xxi. p. 52). The watchglass was gently removed from the flask No. 1, and the dropping-tube inserted; after some minutes a drop fell from the tube, the ether in spreading over the surface described its cohesion figure, and the solution crystallised immediately. In Nos. 2 to 5 the dropping-tube was lowered so as to deliver the drop very gently, or it was allowed to trickle down the side of the flask. In two cases the ether evaporated and left the oil on the surface in minute lenses; there was no crystallisation even on gently shaking the solution. In No. 4 a film was formed on the side of the flask; on inclining it so as to bring the solution into contact with it, crystallisation set in. In No. 6 a film was formed on the surface, but on gently shaking the flask the solution became solid.

*Dust in the Air.*—At the British Association Mr. C. R. Titchborne gives an account of his later experiments on the Dublin atmosphere. His observations, so far as they go, seem to point to a curious phase of the subject—that is, that dust taken at a great height, and in such a position as in certain experiments, should appear to have as great, or greater activity, than that which would be obtained from a building which is nightly crowded to suffocation. This, in some measure, may be due to the extreme levity of the

spores, which are supposed to be the life of the dust, and which lightness may be described as almost approaching volatility. *There is, probably, an altitude of the maximum of activity for all localities as regards dust.* It is so light that even that obtained in an ordinary house contains a large portion that refuses to sink when thrown upon water; and, even when the vessel is placed beneath an air-pump, a large percentage floats. To him the activity of the dust taken from the top of the monument 134 feet high is something marvellous—this source so far removed from the busy streets—yet its organic matter contains what is capable of splitting up, in a short time, hundreds of times its own weight.

*Meteoric Dust in Snow.*—This is a curious subject, but it has been very well followed out by Herr D. Huseman, who in the "Neues Jahrbuch für Pharmacie" (September) gives an account of a reddish-coloured dust which fell along with snow in the Swiss canton Du Vaud in the winter of 1867. According to a calculation, made from experiments and observations conducted with care, the quantity of dust fallen over the entire surface of the canton amounted to about 1,500 tons. The author examined the dust, as well as the snow which had fallen simultaneously. The water yielded by the snow contained a considerable quantity of sulphate of lime and organic matter, both of which are absent in ordinary snow-water, at least in Switzerland. The microscopical inspection of the dust proved it to contain minute particles of mica, felspar, quartz, and variously shaped organic matter. After having been dried at 100°, and ignited, the loss amounted to from 20·8 to even 24 per cent. for four different assays. The greater portion of this loss was due to the volatilisation of water, of crystallisation and constitution, but nitrogenous organic matter was also found to be present. Of the residue after ignition, about half was found to be soluble in hydrochloric acid. This solution, which was only qualitatively tested, contained peroxide of iron, lime, alumina, magnesia, and sulphuric acid; the portion insoluble in acid, having been fused with a mixture of potassa and soda, was found to contain, beside a large quantity of silica, also alumina, oxide of iron, and lime. The dust, when treated with an acid, gave off carbonic acid largely.

*Gas and Gas-works.*—A paper entitled "Instructions, Rules, and Regulations concerning the Use of Gas and the Inspection of Gas-works, Gas-meters, and Gas-pipes, ordered to be observed by the Communal Authorities of Karlsruhe" (Baden), which should be consulted by gas makers and engineers, will be found in the September number of the "Journal für Gasbeleuchtung."

*Experiments in Sub-permanent Magnetism.*—The following experiments are described in the "Chemical News" of November 25. The object of the experiment is to produce, in a few minutes, what Dr. Tyndall has named sub-permanent magnetism; and thus represent to a class *quickly* what is effected by the earth *slowly* in soft iron lying in the magnetic meridian, and subject to molecular disturbance from percussion or other causes. The requisites for the experiment are—a block of cast iron (wrought iron might, perhaps, do) slightly magnetised, a bit of soft iron wire, a hammer, and a magnetic needle for testing the wire. *Expt.* 1. Lay the iron wire on the block, and hammer it lightly from end to end, for a few seconds. Presented to the needle, the wire will be found magnetised, showing distinctly strong north

and south poles, produced by the south and north poles of the block. *Expt.* 2. Place the wire *reversed* on the block, *i.e.* lay the north pole of the wire on the north pole of the block, and hammer as before. Tested again by the needle, the wire exhibits its poles reversed. *Expt.* 3. Lay the wire as in Expt. 1., and hammer; the original polarity is restored. Finally, by changing the position of the wire, the pole may be changed and rechanged as long as the wire lasts. This experiment would seem to represent well the magnetising action of the earth. The block personates the earth with its magnetism, which is not less comparatively than that of the cast iron. Were the wire to remain for a considerable time lying on the block, it would be magnetised. The hammering effects this *quickly*.

*Obtaining High Temperatures in Liquids.*—A very useful invention of Mr. Coffey is now to be seen in operation at Messrs. Doulton and Watts's, of Lambeth. It is a new mode of obtaining high temperatures for the evaporation of liquids without the use of high pressure or superheated steam, and is, in fact, a modification of the circulating system, heated water being replaced by heavy paraffine oils. These circulate exactly like water. A close system being made, the oil heated in a coil of pipe placed in a furnace rises first to an air-tight tank, from which it runs through pipes and the jackets of pans, descending as it cools to the coil of pipe in the furnace. With this apparatus a temperature of 600° or 700° F. may be safely maintained without any of the risks arising from the use of steam at high pressures, and, as will be easily seen, with a much less expenditure of fuel.

*A Severe Test for a Lightning-Rod.*—According to a recent number of the "Boston Journal of Chemistry," a powder magazine at Venice, containing 300,000 kilogrammes of gunpowder (about 300 tons) was struck by lightning this summer. The platinum point of the lightning-rod was melted, and the rod split and twisted, but the electric charge was safely conducted to the earth without doing any other damage. That lightning-rod may be said to have saved a city, for the explosion of such a quantity of powder would have laid all Venice in ruins.

## ZOOLOGY AND COMPARATIVE ANATOMY.

*Objections to Darwin's Theory of Fertilisation through Insect Agency.*—At the American Association for the Advancement of Science Mr. Thomas Meehan read a paper on this subject. He said that the discoveries of Darwin had disclosed wonderful apparent arrangements for fertilisation through insect agency; but occasionally instances were found where, with the most perfect facilities, insects seemed to make no use of them. These had been considered as objections to a full acceptance of Mr. Darwin's theories. The Salvia was an instance. The lower division of the anther acted as a petaloid lever, closing the throat of the corolla tube, which *ought* to throw the pollen on the back of the bee when it entered for the honey. The principle was perfect. *But no insect is seen to enter.* On the other hand, the humble bee, "without which," Darwin says, "some species would die out in England," bores a hole on the outside, through which it gets the honey. The humble bee thus seems to avoid its duties here. A similar state

of things exists in the Petunia of our gardens. The humble bee extracts the honey by making a slit in the tube, and avoids interference with the pollen. But Mr. Meehan found that these flowers are the favourite resort of Sphinxes and other night moths, which do extract the honey from the mouth of the tube, and thus cross fertilise. It would thus seem that plants not only do as a rule prefer fertilisation by insect agency, but probably some classes of flowers have their preferences for certain classes of insects. In the case of Salvia, probably some insects peculiar to their native countries fertilise them ; especially is this probable, as in cultivation the Salvia produces very little seed.

*A New Form of Leech.*—At a late meeting of the Academy of Natural Sciences, Philadelphia, Professor Leidy exhibited in a vessel of water numerous living specimens of a leech, which he said was abundant in the vicinity of Philadelphia, but appears to be an undescribed species. He had first observed it in a pond, on the Delaware, near Beverly, Burlington Co., N. J., from which he obtained the largest specimens. It was found especially beneath half-submerged dead limbs of trees, sometimes between the bark and wood, and in crevices and holes of the latter made by insects. It was also found in the Delaware and Schuylkill rivers near shore, beneath stones. In ditches below the city, and communicating with the rivers mentioned, smaller leeches, apparently the young of the same, were frequent between the leave sheaths of submerged stems of aquatic plants, such as *Zizania aquatica, Scirpus fluviatilis, Sagittaria, Sparganium,* &c. When disturbed, the animal receded from its position of rest, and swam rapidly like the ordinary medicinal leech, *Hirudo decora.* It appears to belong to a different genus from the latter, and approaches most in character *Nephelis,* though it even exhibits points of difference from this as ordinarily described. He has given it the name of *Nephelis punctata.*

*The Fer-de-lance of Martinique.*—A remarkable instance of the development of this species was recorded lately by Professor Cope, who called attention to a large specimen of a *Trigonocephalus,* of which some fourteen inches were enclosed in the œsophagus and stomach of a larger *Oxyrrhopus plumbeus.* The specimens were from the island of St. Lucia, West Indies. He stated that a species not distantly related to the latter (*Ophibolus getulus*) was said to have a similar habit of devouring our native *Crotalidæ.* The islands of Martinique and Guadaloupe had become so infested with the fer-de-lance, *Trigonocephalus lanceolatus,* as to be in parts almost uninhabitable, and it was chiefly on account of the danger from this venomous reptile that collecting naturalists had of late years so seldom visited them. The annual number of deaths in Martinique from this cause was said to be very large. Some means had been adopted to check the increase of this pest, but with small results. Professor Cope thought that, as the *Oxyrrhopus plumbeus* was very numerous in Venezuela and Brazil, and since it was very harmless and easily procured, its introduction in large numbers into Martinique, &c., would be a simple matter, and one probably to be attended with good results in the diminution, at least, of this enemy of agriculture.—*Proceedings of the Society of Natural Science at Philadelphia.*

*The Anatomy of the Panda.*—At the meeting of the Zoological Society on Nov. 15 Professor Flower read a memoir on the anatomy of the Panda

(*Ailurus fulgens*), as deduced from a specimen of this animal which had been presented to the Society by Dr. Simpson, in May 1869, and had lived for some time in the Society's gardens. After an elaborate examination of every part of this animal, Professor Flower came to the conclusion that it belonged to the Arctoidean group of the carnivores, and was most nearly allied to the racoons and other members of the family *Procyonidæ*.

*Notes on Tortoises.*—Dr. J. E. Gray, F.R.S., with an energy which we fancy few of our younger naturalists possess, read, at the Zoological Society on Nov. 1, no less than six communications on various points connected with the natural history of the Testudinata. The first of these contained notes on three tortoises living in the Society's gardens, one of which was believed to be new to science, and was proposed to be called *Testudo chilensis*. The second contained descriptions of two new species of Indian tortoises in the collection of Mr. T. C. Jerdon. The third related to the family *Dermatemydæ*, and embraced the description of a species of this group living in the Society's gardens. The fourth contained notes on a West African river tortoise (*Cyclanosteus senegalensis*), also living in the Society's gardens. The fifth contained notes on *Bartlettia*, a proposed new genus of fresh-water tortoises, belonging to the family *Peltocephalidæ*, and the sixth notes on the species of *Rhinoclemmys*, in the British Museum.

*Anatomical Characters of Limpets.*—In the "American Naturalist" for November Mr. Dall gave an account of the anatomical characters of the conical univalve mollusks generally known as limpets. These have been divided by Gray and other naturalists into two orders, according as the animal possessed one plume-shaped gill over the back of the neck, or a cordon of lamellar gills all around the body. His recent investigation of the anatomy of many species, principally from the American coasts, had shown that the value of these distinctions was less than had been heretofore supposed. Some of the limpets were shown to be entirely without special gills; others possessed a cervical plume-like gill, and also a cordon of accessory gills, greatly varying in extent in the different genera. For this reason he proposed to include them all in one order (named *Docoglossa* by Dr. Troschel), subdividing it into two sections characterised by the total absence, or by the presence, of gills. These sub-orders would respectively bear the names of *Abranchiata* and *Proteo-branchiata*.

*The Development of Discina.*—This subject has been well studied by Professor Edward S. Morse. Referring to his former papers in the early stages of Terebratulina, and the evidence then adduced of the proofs of the close relations existing between the Brachiopoda and the Polyzoa, he said that an examination of the early stages of Discina showed the same simple lophophore, sustaining a few cirri, the stomach hanging below, and other features in which a resemblance was seen. The perivisceral wall is made up of two layers of muscular fibres which cross each other, giving it a reticulated appearance. While the young shell is oval in shape there is marked out a perfectly circular area, indicating that at the outset the embryo possesses a circular plate above and below. The muscles were very large, and occupied most of the perivisceral cavity. The setæ fringing the mantle were very long, those from the anterior margin being nearly three times the length of the shell. The mantle margin, the blood lacunæ, and the bands of muscles to move the setæ, were all described.

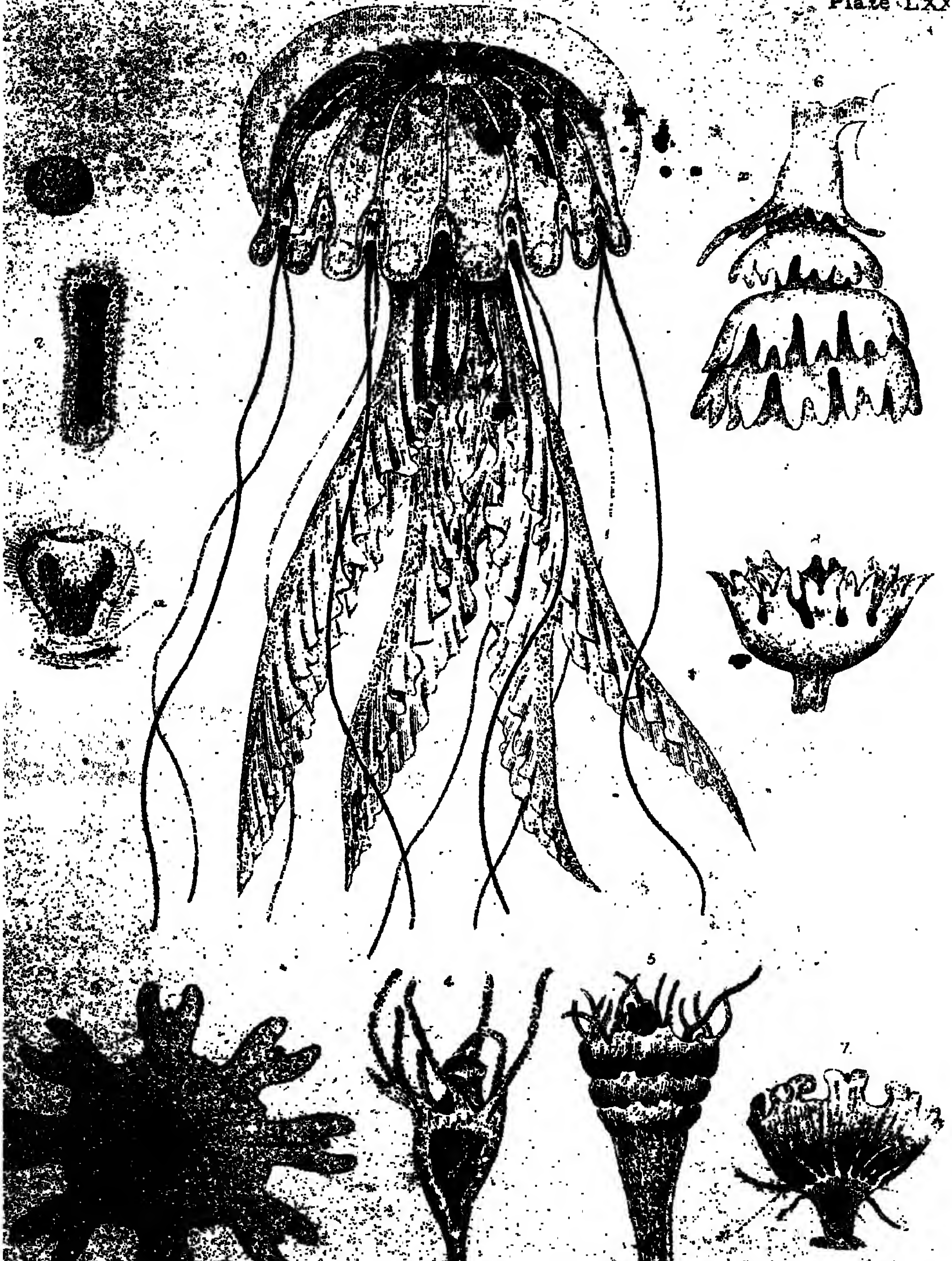
*Salmon in Japan.*—The "Society of Arts Journal" says that Mr. Troup, acting-consul at Niagata, in a report this year to Sir H. Parkes, states that great quantities of salmon are caught in all the rivers of that province. Besides in the Shinano-gawa, it is found in the Aga-no-kawa, the Arakawa, and the Miomote-gawa, or Murakami rivers to the north-east. Might it not be possible to introduce the spawn into Tasmania and New Zealand from Japan more successfully than has yet been done from England?

*Collection of Venezuelan Birds.*—On November 15 a paper was read before the Zoological Society by Messrs. Sclater and Salvin on the recent collections of Venezuelan birds made by Mr. A. Goering in the vicinity of Merida. The present collection was stated to embrace examples of 105 species, nine of which were considered to be new to science. Amongst the latter were two new parrots, proposed to be called *Urochroma dilectissima* and *Conurus rhodocephalus.*

*The Relations of Brachiopods and Worms.*—It seems unlikely that the Brachiopods should be classed with the Annulosa, yet they have been; and we are glad to see that an American naturalist, Mr. Dall, places the Brachiopods once more among the Mollusca. In doing so he referred to several special points of structure, especially the peduncle of Lingula, demonstrating its construction to be analogous to that of the siphons of bivalve mollusks, such as the common clam, *Mya arenaria.* He then described the bristles of Lingula, showing that they were quite different in construction from those of the worms, and also that the Chitons were (in some genera) provided with true follicular setæ, proceeding from the mantle. Hence these characters cannot be held to afford satisfactory evidences of affinities with Annelids. Mr. Dall then proceeded to discuss the theory of Mr. Morse, that the Brachiopods were a subdivision of the Annelids. Mr. Dall took the opposite view, and, says the "American Naturalist," while admitting all the facts brought forward by Mr. Morse, and fully appreciating the careful and thorough nature of his researches, contended on the other hand that these facts were susceptible of quite another interpretation. Mr. Dall then went on to take up, one by one, the circulatory, nervous, muscular, and digestive systems of the Brachiopods, and to compare each with the same organs in the Annelids and the Mollusks, and came to the conclusion that the weight of structural characters was essentially of a Molluscan nature. The Mollusks were an individualised type, while the Annelids, and even most of the Articulates, were typified by their repetition of similar organs. No such repetition obtains among the Brachiopods. Mr. Dall was of the opinion that the Molluscoidea should rank as one of two great primary divisions of the Mollusca—one, the true Mollusks, typified by the Gasteropoda, and second the Molluscoidea, typified by the Brachiopoda. The second division would include the Polyzoa, Tunicata, and Brachiopoda; and Mr. Dall was of the opinion that these groups were essentially related to one another, and cannot be separated without violence to their affinities.

*The Cranium in Reptiles, Batrachia, and Fishes.*—Professor E. D. Cope read a long paper before the American Association on this subject. It is reproduced, with the woodcuts, in the "American Naturalist" for October. It is a lengthy and important paper, and we do not do more than refer to it here, for it would be impossible to give any abstract of it whatsoever.

Plate LXX



Development of the Discophora.

# THE DISCOPHORES, OR LARGE MEDUSÆ.

BY THE REV. THOMAS HINCKS, B.A.

[PLATE LXX.]

FEW things can be less attractive or suggestive of grace and brilliancy than the great lumps of jelly which so often strew the beach at certain seasons of the year. Yet these are the remains, sorely mangled in the rough collision between sea and shore, of some of the loveliest of living things. A jelly-fish stranded, soiled by its contact with the sand, mutilated by the rush of the angry waters, its crystalline lustre dimmed, its vivid colours faded, its rhythmical movement stilled, is an unsightly ruin. Swept in by the advancing tide, with no power of resistance, and left an inert mass upon the shore, it is the very type of utter helplessness. But the Medusa, floating on the surface of a calm, clear sea beneath a summer sky, or gently borne onward by the rhythmical pulsations of its swimming-bell, its long fringe of slender filaments dependent from the margin of its crystal dome, and trailing after it in graceful curves, is a model of exquisite form, and suggests nothing but happy freedom and the very luxury of motion, as it rises and falls in the yielding water. To the other elements of beauty which characterise it, vivid and varied colouring must be added; the hyaline disc is often adorned with blue, yellow, rose, purple, and other tints, while the fringes are also richly dyed.

We are not surprised to find that these beautiful creatures engaged the attention of the ancients, and have always excited the wonder of naturalists, if they have not always received very intelligent treatment at their hands. The singularity of their forms, and the profusion in which they occur in all seas, were sure to arouse curiosity at least, while the striking peculiarities of their structure and mode of life have stimulated and baffled the research of the anatomist and physiologist. "All seas," say MM. Péron and Lesueur, whose names are most honourably

connected with the history of the tribe, "produce various kinds of these singular animals; they live amidst the Arctic waters of Spitzbergen, Greenland, and Iceland; they swarm under the fires of the Equator, while the great Southern Ocean is rich in numerous species. All maritime peoples appear to have known them from the highest antiquity; Philippides, Eupolis, Aristophanes, and Diphilus, before Aristotle, have mentioned them; and from the time of Pliny to our own days (the beginning of the 19th century) more than one hundred and fifty writers of all the nations of Europe have occupied themselves with their history."* The true interpretation, however, of Medusan structure and life has been reached since the French naturalists wrote.

Aristotle, singling out a character which is by no means universal, fixed upon the tribe the name of sea-nettles (*Acalephæ*), in conjunction with the Actiniæ; and it has clung to the Medusæ from his time to the present. Many of the species, no doubt, sting, and sting severely; but many more seem to be destitute of the power, or to possess it in a very slight degree. The secretion of a poisonous or paralysing fluid, it may be remarked in passing, has an important relation to the *diet* of the jelly-fish, enabling it to deal with creatures much higher in the scale of being, and much more strongly built than itself. Other popular names for the Medusæ are founded on the phosphorescent properties which many of them manifest, and which form so striking a feature of their history. To the Italians they are "*Candellieri di mare*;" the Arabs call them "*Kandil el bahr*" (*lucerna marina*). The resemblance between the rhythmical pulsation of the disc and the respiratory movement has suggested the designation of *Pulmo marinus*, or "sea-lungs," while our own "jelly-fish" and its less refined equivalent "sea-blubber," if not so poetical, are in their way as expressive as any.†

But turning from names to the things themselves, the *Discophora* constitute an Order of one of the two great branches into which the Cœlenterate sub-kingdom divides itself, the *Hydrozoa*. It ranges alongside the Hydroid Zoophytes, as a parallel group; and while the two divisions have their capital features in common, they are separated by a sufficiently broad line of structural difference. The Hydroid includes within the compass of its individuality two principal elements, a fixed alimentary zooid or hydra, and a reproductive zooid, which is frequently organised for free and independent existence, on the Medusan

* "Histoire générale des Méduses." Introduction.

† Other trivial names are "Gelée de mer," "Chapeau marin" (Mediterranean), "Boule de mer," "Capello di mare," &c.

type, but also frequently maintains its connection with the primitive stock in a less highly specialised form. So the Discophore has its two equivalent elements; a fixed polypite, whose functions are merely nutritive and vegetative, and a sexual zooid, developed from the former by gemmation, in which the Medusan structure reaches its highest grade, and which always leads a free oceanic life, while discharging its reproductive functions. In both groups there are exceptional genera in which the course of the life-history is modified by the suppression of the fixed element, and the Medusa is developed directly from the ovum, and not as a bud on a hydraform stock. Such forms, however, exhibit in each case the closest affinity to those which are produced in a more normal manner, and should clearly take their place amongst them in the ranks of the same division. In the two groups, then, the general plan of the life-history is identical, and there is a striking similarity in many of the structural features. The differences will be noticed when I come to describe the organisation of the Discophore in detail; but one salient point of contrast may be mentioned at once. Amongst the Hydroid Zoophytes the fixed vegetative element predominates; the sexual members of the colony, though sometimes free and locomotive, are in a large number of cases as permanently attached to the parent organism as the flower to the plant, and where medusiform zooids are present, they are comparatively small, inconspicuous, and of a lower structural grade. But amongst the Discophores, the locomotive medusan element is altogether in the ascendant and reaches its culminating point; the polypites are small and insignificant, and of much the same pattern; the more highly specialised structure has gained upon the merely vegetative; and vagrancy, instead of plant-like fixity, is the characteristic of the tribe.

Let me first define more precisely the limits of the Order which forms the subject of the present paper. The *Discophora* embrace the large jelly-fishes (Plate LXX. fig. 10), or *Swimming-polypites*—to borrow the expressive German name—whether developed directly from an egg or as buds from a hydra-like stock; and also an aberrant group of fixed Medusæ, the *Lucernariidæ*, of which I shall have more to say hereafter. The jelly-fish is both the most characteristic and the most familiar form under which the Discophore presents itself to us; and I shall at once attempt to sketch the leading features of its structure in plain as distinguished from technical language, merely premising that in a large proportion of cases it is not to be regarded as in itself a *perfect animal*, but only as one term of a life-series, which cannot be rightly interpreted alone.

The feature which is most prominent and at once arrests

attention is the ample gelatinous disc, the mass of "blubber," as it appears when cast upon the sands—the contractile bell, or swimming-organ, of more or less transparency and most graceful proportions, by whose rhythmical movement the living Medusa is propelled through the water. This is both the locomotive organ and the float on which are suspended the various organs of prehension and digestion. It varies in form, though scarcely in grace, but is commonly a glassy hemisphere with lobed or sinuated margin, from which in many cases depend a multitude of fringe-like filaments, or a smaller number of long extensile arms (vide Plate LXX. fig. 10). These tentacular appendages are sensitive feelers, cast about in all directions in quest of prey, and also fishing-lines by means of which it is arrested and dragged towards the mouth. In some species they are present in enormous numbers, and are capable of extrordinary elongation. The gigantic *Cyanæa arctica*, which sometimes attains a diameter of 7½ feet, has these organs disposed in eight bunches round the margin of its disc—tangled masses of interlacing threads, in constant motion, which can be extended to a length of more than 120 feet! * When it is remembered that this formidable offensive apparatus is endowed with the power of stinging violently, that these extensile filaments, which can be shot out to such amazing distances, are poisonous and paralyse as well as grasp, we may form some idea of the terrors that wait upon this floating mass of jelly.

Round the margin of the disc are ranged certain organs of vision, which may be regarded as the equivalent of the eye in more highly organised beings (Plate LXX. fig. 9*a*). Each of them consists of a spherical cluster of lenses, borne on a peduncle, and usually more or less protected by a hood-like covering. Professor H. J. Clark, who has carefully studied the intimate structure of these bodies—who has actually taken out the minute lenses and turned them about, so as to trace the curvature of the face—is of opinion that we have in them "all the elements of an optical apparatus sufficient to produce a distinct image." At any rate, there can be little doubt that they are light-perceiving organs, and serve in some way or other to direct the creature in its course. Agassiz remarks that "there can be no doubt that these animals perceive what is going on about them, and that they are very sensitive to changes in the condition of the atmosphere. . . . . Even accidental disturbances are perceived by them, for when approached, however carefully, the change of their course, or the unusual rapidity with which they sink, show plainly that they are making the utmost efforts to escape. . . . . When approached with a dip-net it is evident, from the

* A. Agassiz, in his "Catalogue of North-American Medusæ."

acceleration of their movements, that they are attempting to escape."

From the centre of the lower or concave surface of the disc—the top of the domed cavity—is suspended the digestive sac, a somewhat four-sided, proboscis-like body, terminating in a quadrangular mouth, which we readily recognise as a polypite, in spite of its disguise in the adaptive dress that fits it for a locomotive existence. The angles of this mouth are extended into lobate processes, often of very considerable length (Plate LXX. fig. 10), which are hung with fringes and furbelows, and form a striking feature of the organism, projecting as they often do far beyond the opening of the swimming-bell. These appendages, which have been styled the "grasping arms," assist in securing the prey and conveying it to the mouth. They also in some cases subserve another and very different purpose; within their ample folds, marsupial pouches are, as it were, extemporised, in which the ova pass through certain stages of their development, and ripen into the perfect embryo. At its upper extremity the pendent digestive sac opens into a somewhat extensive cavity, from which a certain number of tubular prolongations are given off, which penetrate the substance of the disc, and, after dividing, and in some cases sub-dividing, and anastomosing, so as to form a complicated vascular network, terminate in a circular canal that runs round the disc a little within the margin. Through these tubular offshoots of the stomach, radiating through the inferior stratum of the swimming-bell, its contents are at once distributed, and applied to the nutrition of the whole structure. They compare with the central channel, traversing all the ramifications of the common flesh in the plant-like zoophyte, and communicating directly with the stomachs of its multitudinous hydræ, by which the prepared pabulum is conveyed throughout the length and breadth of its complex organism.

In close connection with the central cavity, just described as surmounting the digestive sac, and also with the radiating tubes concerned in the circulation, are found certain pouches within which the ovary and spermary are lodged. These pouches open into the body-cavity, and the ova pass into it in due time, and find their way through the mouth to the marsupia prepared for their reception during the further stages of their development. The deeply-coloured reproductive organs show distinctly through the transparent disc, and give rise to the cruciform figure which adorns the summit. So much may suffice in the way of structural detail. We have now the grand features of this organic type clearly before us.* It is less easy to describe the various

* I have not referred to the partially developed membranous veil which in some species surrounds the margin of the disc, nor to the curious internal

beauty of form; to give an idea of the exquisite motion of the bell, pulsating with rhythmical regularity, and, by its alternate contraction and expansion, driving itself gently through the water; or of its equally exquisite rest, as it floats, balloon-like, upon the tranquil surface; to trace the curves of the flexile tentacles, or to paint the delicate but vivid hues with which the glass-like fabric is tinted. Let us hear M. Lesson, who, whatever may be his merits as a classifier, manifests the true enthusiasm and sensibility of a naturalist in writing of his favourite tribe:—"Il est peu d'animaux plus variés et plus intéressants à connaître que les acalèphes. . . . Ils rivalisent avec les fleurs par l'éclat de leur coloration. Souvent les gemmes ne scintillent point avec plus d'éclat que certains d'entre eux. . . . Vaguant solitaires ou par essaims de myriades d'individus sur la surface des mers par le temps de calme, cachés lors des orages ou lorsque les vagues se heurtent; et, cherchant un refuge dans les couches d'eau plus paisibles, ils viennent pendant la sérénité des nuits émailler le bleu azuré de la mer par une phosphorescence vive et merveilleuse.'*

The distinguished naturalist, who, under the pseudonym of Alfred Frédol, has given us so charming an account of "Le Monde de la Mer," thus refers to the coloration of the Medusæ: "Sometimes the animal is colourless and of a transparency almost equal to that of crystal; sometimes it is slightly opaline, of a delicate blue, or a pale rose colour. In some cases it presents the most vivid tints and the most brilliant iridescence. In certain species, the central portions only are tinted red or yellow, blue or violet; the rest of the body is without colour." These are suggestions of a beauty which really cannot be described; for, after all, it is colour suffusing the most exquisitely delicate tissues, and in association with perfect grace of form and motion that constitutes the charm of these ocean-wanderers.

A word on the luminosity of the Medusæ. The *Discophora* are by no means principal agents in producing the phenomena of phosphorescence. Certain kinds only are luminous in any high degree, and though the large and brilliant ball is a striking feature in the general illumination, it must yield to the myriads of *Noctilucæ* and other minute beings which enamel the surface of the ocean "like little constellations fallen from the skies," or cover the dark waves with soft yellow light, and

tentacles (?) which are grouped about the generative pouches, and constitute a distinctive characteristic of the Discophora, because my object is to fix attention upon the *obvious* peculiarities of the tribe, rather than upon structural minutiæ, however important.

* "Histoire naturelle des Zoophytes. Acalèphes, par René-Primevère Lesson."

change the very foam into "sparkles of sea-fire."* The Medusa represented in our plate (*Pelagia cyanella*) is eminently phosphorescent. We owe some of the best observations we possess on the subject to Spallanzani, who had the opportunity of studying a luminous species of Discophore on the coast of Sicily, and has given us in his "Travels" an interesting account of the results which he obtained. He found that the light was intermittent, sometimes continuing for a quarter of an hour, half an hour, or more, and then being suddenly extinguished, and not reappearing for a considerable interval. He was led to believe that the phosphorescence was manifested strongly only so long as the Medusa oscillated uninterruptedly, and faded when it passed into a state of rest. He also ascertained that the light was more vivid during the contraction than during the expansion of the disc, and that the principal seat of it was the edge of the swimming-bell and the large tentacles. This localisation of the phosphorescence has been noticed in many of the hydroid medusiform zooids. In one species the light kindles in the central proboscis alone, and resembles a little lamp suspended in a crystal globe. Amongst the fixed polypites the phosphorescence is fitful in its manifestations; they kindle their lights when irritated, and quench them when left to themselves. Of the mode in which the luminosity is produced, and its precise relation to the economy of the animal, we know little or nothing.

The development of the Discophore is the next point which claims our attention; and though the remarkable series of facts first brought to light by the independent researches of Sars and Dalyell has become so familiar as to have lost much of its marvellous hue, the real interest of it is still fresh as ever; it still reads like a romance to the uninitiated, while the naturalist has, perhaps, hardly fathomed its full significance. As I have mentioned, there is some variation in the course of development. I propose to follow the line which may be regarded as normal and characteristic of the tribe, and in doing so to take special note of the parallelism between the history of the Discophore and of the Hydroid, and also of the points of divergence.

The Medusa, then, as we have seen it, is fully equipped for the discharge of the reproductive function; and at the proper season the laden ovaries yield up their contents, and the embryos make their way from the generative chamber through the digestive cavity and its oral opening, and lodge themselves (how it is hard to say) amongst the ample folds of the tenta-

* The phrase is Nathaniel Hawthorne's, who had the keenest eye for all the aspects of nature, and who thus describes the phosphorescence, which he had witnessed from the deck of a steamer off the American coast.

cular fringes. There they complete their development, and finally leave the parental shelter, as ciliated, free-swimming planulæ. (Plate LXX. fig. 1, *the ovum*; fig. 2, *the embryo.*) In this condition they bear an exact resemblance to the corresponding term of the Hydroid life-series. They appear as cylindrical bodies, thickly clothed with vibratile cilia, and furnished with a mouth at one extremity. After a term of free existence the planule selects a site for permanent settlement,* and, having made a suitable choice, fixes itself by the pole of the body opposite to that which bears the mouth, and exchanges its roving habit for a purely vegetative life (Plate LXX. fig. 3). Its shape and proportions have altered; it is now narrowed below, and expands above; the ciliary appendages are still retained for a time, but they have lost their activity, while around the extreme basal portion of the body, a very delicate film of chitine is in some cases soon developed (Plate LXX. fig. 3*a*). The presence of this horny sheath, which, according to Agassiz, is only characteristic of some species, is a very interesting point, as another feature common to the Discophores and the plant-like Hydroids. As development proceeds the body lengthens, the cilia totally disappear, the upper portion becomes broad and somewhat cup-shaped, and from its margin bud a number of thread-like arms, while in the midst of them rises a quadrangular proboscis, bearing the mouth on its summit. The product of the Medusan egg now appears as a well-developed polypite, the equivalent of the primary zooid of the Hydroid colony (Plate LXX. fig. 4). Yet with all the similarity between them, there is an element of unlikeness too. The rather deep, cup-shaped disc surrounding the prominent proboscis of the Discophore suggests the swimming-bell of the Medusa, and we feel that if it were turned adrift, to float on the water, with the mouth downwards, it would bear no slight family resemblance to its parent.† In a word, the polypite of the Discophora has more of a Medusan look than that of the Hydroids.

To pursue the history, the polypite multiplies the number of its arms until they become a goodly company, corresponding with the voracious appetite which it is their office to satisfy; they are very extensile, and when much elongated are like threads of gossamer waving through the water. The mouth is

* This process of selection, often conducted with much apparent fastidiousness, may be witnessed in the case of the embryos of fixed animals generally.

† It must be remembered that some of the hydroid polypites have the tentacles connected by a web-like membrane, plainly indicating the way in which the structural elements are modified, so as to convert the fixed hydra into the free medusan or sexual zooid.

a striking feature of the organism; it is kept in frequent motion, as if on the watch for prey, and is often thrown wide open, so that every recess of the capacious stomach becomes visible. The polypite is very protean in shape, and never long the same. As it feeds and attains its full proportions, its vegetative powers begin to manifest themselves; young sprout from various parts of the body, as in the Hydra, and several generations are at times organically united; long, thread-like shoots are also cast out, from which new polypites are developed, and soon the primary zooid is the centre of an extensive colony that has literally grown out of its own substance.

But as the seasons change it enters upon a new phase of its being; the production of polypites like itself ceases, and a new developmental process sets in. The body, which is now large and cylindrical, begins to divide across; first a constriction a little below the tentacles, then another a little below this (Plate LXX. fig. 5), then another, and so on till the whole is partitioned into transverse segments, with the exception of a small portion of the base. The constrictions deepen; each segment becomes more and more independent, while its margin is cut into prominent, sinuated lobes, which show like frills on the surface of the now disintegrated body. In this state the structure presents the appearance of a pile of circular discs; or we may compare it, with Agassiz, "to a string of lilac-blossoms, such as the children make for necklaces in the spring." But the polypite is not to lose its identity. Immediately below the *lowest* segment a new circle of tentacles is developed (Plate LXX. fig. 6*x*); about the same time the original set at the top is absorbed and disappears;* the segments, now connected by the slightest link, begin to manifest independent vitality, and exhibit the contractile movements so characteristic of the Medusæ. After a term of vigorous struggles the uppermost frees itself from its connection with the polypite, and the rest soon follow. The basal portion, with its new wreath of arms, survives. The detached segments, reversing their position in the water, present the pulsating disc of the Medusa, with its lobed margin and circle of eyes, and take to the customs of free oceanic life (Plate LXX. figs. 8 and 9). Let us pause for a moment to consider the significance of this course of development. The polypite of the Discophore, after multiplying itself indefinitely by gemmation, proceeds to fulfil its most important function in maturing certain highly specialised reproductive buds, which it casts loose at a certain point of their development, to lead an independent life and prepare and distribute the seed of new generations. We have a parallel

* Van Beneden's "Polypes," pp. 80, 81.

series of facts in the history of the Hydroid. The difference lies simply in the *mode* of gemmation. The hydroid Medusa buds from the side of the polypite, or of a special zooid, or of some portion of the common flesh; the Medusa of the Discophore is a bud formed by the transverse division of the body of the polypite. And it may be remarked that this *mode of budding* is not altogether unknown amongst the Hydroids, for the Hydra has been observed to multiply by transverse fission. The marvel of the staid polypite resolving itself into a company of mercurial jelly-fishes disappears; like the plant, it has only put forth its flower-buds, with the difference that they open into full bloom and mature their seed *apart from the parent organism.*

I shall now sum up briefly the chief points of difference between the two Orders. Amongst the Discophores, the free locomotive element predominates; the fixed plant-like element amongst the Hydroids. The polypite of the former makes a nearer approach to the medusan form than that of the latter. The sexual zooids (*Medusæ*) of the Discophores possess solid and massive discs, with lobed margins and pedunculated eyes often protected by hood-like coverings, and a complex anastomosing system of vessels; the opening is rarely provided with a veil, and, when present, it is very slightly developed; the tentacular appendages of the mouth attain an extraordinary size, and the reproductive organs are lodged in distinct chambers, communicating by a definite orifice with the cavity of the body, through which the embryos make their escape. On the other hand, the free sexual members of the Hydroid colony are comparatively small, and have fragile and filmy bells, provided with an ample veil; the vessels are generally simple; the eyes sessile, unprotected, and of a humbler type; the furbelowed oral appendages are wanting; and the generative products are lodged between the outer and inner wall of the digestive sac or of the radiating vessels, and are liberated by the rupture of the parts. And, lastly, the mode in which the medusiform bodies originate and develop themselves differs in the two divisions. Many of these distinctions have little special significance; and though there may be enough amongst them to justify us in separating the two groups in our classification, it must be remembered that the affinities are of the closest and most intimate kind.

To resume our history. The polypite having dismissed its brood lives on. Thanks to its voracious appetite, it soon repairs the waste of its substance, and in the following spring may, so to speak, bud and blossom again. The young Medusæ, which are liberated in a very immature condition, pass rapidly through the further stages of their development; so rapidly

that in the course of a short season the minute slice of the polypite has attained the extraordinary size and complexity of the gigantic *Cyanæa*, before referred to. All their energies are devoted to the nutrition, protection, and dispersion of the embryos; and having accomplished this work, they probably fade and perish. They share the beauty, and the frailty and transiency of the flower.

I shall now gather together some interesting particulars of their habits and mode of life. Born in the spring, they swarm at this season in immense numbers near the shore; as the summer advances, and they increase in size, they seem to disperse themselves over the surface of the ocean, congregating again in autumn for the purpose of spawning. In warm, serene weather they keep near the surface, and "wander in the luxury of light;" in storms they sink to the safer depths. Their numbers are simply incalculable; off our own coasts they may be seen in immense shoals. Sailing from Holyhead on a fine evening, I have seen the water of the harbour so densely packed with them that the steamer almost seemed to be cleaving its way through a solid mass. The works of voyagers and travellers are full of marvellous accounts of the crowds of Medusæ which they have encountered. Lesson tells us that off the coast of Peru he met with millions of individuals of a certain species, pressing closely one against the other as they moved along, and all having the disc directed towards the north; the sea was perfectly calm. Dr. Collingwood describes a shoal in the Atlantic. "Just before sunset," he writes, "we passed through them for a space of two hours, during which time we had traversed ten miles. It was easy to calculate roughly that there could not be less than thirty millions of individuals constituting the shoal—an estimate probably far below the mark." We shall hardly be surprised at their numbers, if we bear in mind the facts of their history. Each polypite, by budding, multiplies itself to an amazing extent, and so provides a large family, each one of which will in due time produce its complement of Medusæ; and, further, each one of the second generation multiplies in the same way, and each one of many successive generations, before the development of the Medusa-brood sets in. And of this vast company, the ultimate product of a single zooid, each one that survives may originate a dozen Medusæ or more. Of such myriads no census can be taken.

In autumn the Medusan tribes return from their oceanic wanderings, and congregate near the shore. Massed together in enormous shoals they discharge the embryos, which it has been their function to mature, in the neighbourhood of the littoral region in which they are to find a home. Agassiz has

witnessed a shoal in the act of spawning. "Myriads of specimens had clustered together so closely that they formed an unbroken mass. . . . They were in such a deep phalanx that it was impossible to ascertain how far below the surface they extended, while those in the uppermost layer were partially forced out of the water by those below."

At this time their energies begin to fail; the animal flowers begin to fade, and the autumnal gales strew them on the beach. Their work is accomplished; but the cycle of new life and development which they have originated is already proceeding in the neighbouring waters.

One or two curious peculiarities of habit may be noted. A species has been observed which is nocturnal in its habits: rarely seen by day, it swarms by night at the bottom of the sea, and is recognised by its phosphorescent light as it moves rapidly about. Another (*Polyclonia frondosa*) has been observed by Agassiz on the Florida reef, "groping in the coral mud at the bottom of the water, where thousands upon thousands may be seen crowded together. . . . They crawl about like creeping animals, now and then only flapping their umbrella." This custom, so different from the usual habit of the Medusæ, may connect itself with a peculiarity in the structure of the mouth and oral appendages, and the absence of the long marginal fishing-lines which characterise the section of the Order to which the species belongs (*Rhizostomeæ*). Other naturalists report that they have seen Medusæ lying at the bottom of the sea, with the arms turned upwards, expanded like a flower. This, we may suppose, is their mode of resting.

The Discophores afford some curious instances of "commensalism," that is, the association of two animals for the benefit of one or both of them. Van Beneden mentions a Medusa which has a small fish as a permanent lodger within its body. The fish sails out and returns at pleasure, but finds its home with the Discophore. The fishermen off the coast of Jutland have long observed that a quantity of young fishes are always found beneath the disc of a large *Cyanæa*, and sheltering amongst its long tentacles; they only abandon their retreat when strong and swift enough to protect themselves. Another Medusa (*Pelagia*) is accompanied by a tribe of small fishes—sometimes as many as twenty or thirty—which swim about in the fringes of the oral appendages, finding there both safety from enemies and food. Dr. Collingwood found a small crab residing within the disc of a Medusa, where no doubt he had not only free lodgings, but a share of the crumbs that fell from his host's table. M. Quoy and Gaimard observed a pteropod or winged mollusc living amongst the long tentacles of a *Cyanæa* off the coast of New Holland. A crowd of small crabs and fishes had

also found a refuge in the tangle of interlacing threads. Commensalism, which is comparatively a new subject, would seem to have a wide range in the animal kingdom.

A few words may suffice for the economic uses of the Discophores, which seem to be of the smallest. The old doctors found healing virtues in them, and used them as medicines, with probably as much success as many of their drugs. They have been carted as manure, in ignorance of the fact that the solid matter of a jelly-fish weighing many pounds is represented, when the water is evaporated, by a few grains of film. Somebody has undertaken to extract ammonia from them; and Mr. Arthur Adams has seen the Chinese "cut off huge slices of the firm translucent jelly" of a stranded *Cyanœa* as a relish for the evening meal.

Their best use, after all, is that to which the philosophic naturalist puts them.*

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## DESCRIPTION OF PLATE LXX.

FIG. 1. The ovum of the Discophore, with germinal vesicle and spot.
,, 2. The planula or embryo.
,, 3. The embryo attached—the cilia still remaining; *a*, the horny sheath.
,, 4. The polypite.
,, 5. A polypite dividing transversely.
,, 6. The same, showing the segments in a more advanced condition, and already exhibiting a medusan form; *x*, the new wreath of tentacles.
,, 7. The polypite bearing a single Medusa-bud; the rest of the pile having become free.
,, 8. The young Medusa shortly after detachment.
,, 9. The same, showing the lobes and eyes.
,, 10. *Pelagia cyanella* (Péron and Lesueur), adult. This Medusa is produced directly from the egg without the intervention of a polypite stage.

Figs. 6 and 8 are after Van Beneden; the rest are after Agassiz.

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* No space is left me for the aberrant group of *Lucernariidæ*, which are *fixed Medusæ*, producing their like without the intervention of a polypite.

# THE ISSUES OF THE LATE ECLIPSE.

By J. CARPENTER, F.R.A.S.,

Of the Royal Observatory, Greenwich.

THE eclipse expeditions of December last differed remarkably from others that had preceded them. They differed favourably as regards the elaborate character of the means and schemes of observation, and very unfavourably as regards the success of the observers' intentions. Upon the occasion of the first eclipse expedition, that of 1851, every observer went upon his own account to see what he could, without foreknowledge of what he would see, and without ideas upon the ultimate bearing of any observations he might have the good fortune to make. How successful the expeditionists were in viewing the phenomena may be judged from the fact that nearly a score of accounts of observations were given in the "Astronomische Nachrichten," from astronomers of repute located along the line of totality (which crossed Sweden and Norway), while as many more were included in a special volume of the "Transactions of the Royal Astronomical Society." In the second expedition—that for observation of the eclipse of July 18, 1860—there was again a general vagueness of intentions: everyone was to observe what he could for himself; what little organisation was called for was discussed on board the *Himalaya* on her way to Spain. And how fortunate the observers were on this occasion is known to those who have had to refer to eclipse literature, and who have found records of bewildering extent of the observations made on that propitious day. How differently matters stood in relation to the last eclipse! There was but one subject of inquiry—the constitution of the corona. There were distinct points by which the question was to be attacked, and every observer had allotted to him a definite part of a well-considered scheme of observations. Instead of one man aiming to explain the whole phenomena of an eclipse by himself, as on previous occasions, every man was entrusted with one link in the chain of inquiry. The gazer was to

sketch, the polariser was to study points precisely indicated, the spectroscopist the same, and the photographer was to expose his plates so as to seize upon critical appearances. These were the duties of principals; subordinate labours were as definitely arranged. Never before had there been so extensive and determinate an organisation; and it is quite conceivable that, had the weather favoured the observers, the one outstanding enigma of eclipse phenomena would have been so nearly solved that future eclipses would have possessed little interest for astronomers—a measure of success the desirability of which may perhaps be questioned.

But this was not to be. Ill fortune met the observers at every turn. Deaf ears were turned to their prayers for Government aid; delays were forced upon them; one party was shipwrecked, and another nearly so; one detachment, of which the present writer was a member, had their observing tents and telescopes blown down by a gale, and their most valuable instruments saved from utter ruin as it were by a miracle; and, worst of all, the heavens frowned upon most of the expeditionists on the eventful day. And it is a curious, though an insignificant circumstance, that the weather misfortunes which befell the English observers scarcely affected their American confederates. But the few French and German astronomers who attempted observations were all unsuccessful. M. Janssen indeed deserves a martyr's fame. He escaped from Paris in a balloon, taking with him a silvered glass reflector of 13 inches aperture, and a small spectroscope, and landed at Savernay, whence he pushed on to Oran, arriving there some ten days before the eclipse and living like a hermit the while at his observing station, nine miles from the town, in a desolate and uncomfortable barrack: and all to no effect.

The distribution of observing parties along the line of totality and their measures of success were as follows:—

IN SPAIN.

| | | |
|---|---|---|
| At Cadiz | Lord Lindsay's party . . . . | successful |
| San Antonio | English expedition detachment (under the Rev. S. J. Perry) . . | small success |
| Xeres | American party (under Professor Winlock) | successful |
| Gibraltar | English expedition detachments (under Captain Parsons) . . . | no success |
| Estepona | | insignificant success |

IN AFRICA.

| | | |
|---|---|---|
| At Oran | English expedition detachment (under Dr. Huggins). French observers, Janssen and Bulard . . . . | no success |
| Tunis | Vienna observers (Drs. Weiss and Oppolzer) | no success |

In Sicily.

| | | |
|---|---|---|
| At Terranova | Italian commission (Professor Denza and others) . . . . . | partial success |
| Carlentini | American observers (Professor Watson and others) . . . . . . | successful |
| Syracuse | American observers (Professors Harkness and Eastman) . . . . | partial success |
| | English expedition, detachment (Mr. Brothers) . . . . . . . | successful |
| Augusta | Italian Commission (Padre Secchi and others) . . . . . . . <br> English detachment (under Professor W. G. Adams) . . . . . | partial success |
| Villamonda | English detachment (under Mr. Ranyard) | successful |
| Catania | English detachment (under Mr. Lockyer) | no success |
| Etna | English detachment (under Professor Roscoe) . . . . . . | no success |

In this tabulation the ill fortunes of the English observers are plainly apparent. Yet so far as securing observations is concerned there has been a good deal of success, and in the end it matters little who achieved it. How far the observations will go in settling the questions at issue we shall presently endeavour to examine.

But, in the meantime, let us remember that a solar eclipse is an occasion for observations other than those relating to the sun's constitution and surroundings. When the moon passes between us and the sun, and appears as a black body on his disc, an opportunity is offered of making a determination of the moon's place at a critical part of her orbit. Ordinarily we cannot see the moon for a day or two on either side of conjunction, and the observations which are desirable for determining the errors of her predicted positions at such times are therefore wanting. The continuity of the watch which is kept upon the moon's intricate motions at a great observatory—Greenwich, for instance—is broken at this period of every lunation, except when an eclipse is visible from the observatory, and then the black moon can be observed upon the sun just as the bright moon is observed upon the sky. In less accurate times than the present this observation was simply made by noting the instants of beginning and ending of the eclipse; but there is so much uncertainty, depending chiefly upon the dimensions of the instruments employed, in noting these instants, that the data thus procured are rarely made use of.* Occasionally an old

* Amateurs often betray great anxiety to secure the accurate times of first and last contacts in observing solar eclipses (the late eclipse offered abundant instances). Under the best of circumstances those times are of

eclipse observation is available for fixing the moon's approximate place in the absence of better material, such as meridian determinations. An instance of such application presented itself a few months since, when Professor Newcomb, wishing to ascertain how, within wide limits of error, the present lunar tables represented the moon's place a century and a half ago, resorted to the contacts in the eclipse of 1715 observed by Flamsteed, Halley, and Pound. Such rough observations would be of little avail now.

In order to extract useful data from the passage of the moon over the sun, the Astronomer Royal some years since devised a plan of measuring the cusps in their changing positions throughout an eclipse in such a manner as to bring out the errors of all the numerical elements concerned in the prediction of the phenomenon, the most important of which are the co-ordinates of the moon's position. This method was first put in operation in 1836, and again, with great instrumental power, during the eclipse of July 18, 1860, when the deduced errors of the "tabular places"—as the places predicted by calculation are called—were found to agree closely with those exhibited by observations of the moon which were procurable in the ordinary manner near to the time of conjunction, though there is necessarily a small incomparability from the virtual difference between the black moon on the bright sun, and the bright moon on the black sky; the effects of irradiation being reversed in the two conditions. These observations were repeated at Greenwich during the eclipse under notice, and with a similar resulting agreement between the observed and calculated data.

It will be obvious that they do not require the eclipse to be total at the place of observation. One distinguished mathematical astronomer, however, Professor Newcomb, who is understood to be engaged upon the construction of new tables of the moon's motions, thought it desirable to make the cusp-measures directly upon the line of totality, and he came from the United States for the sole purpose of so making them. He stationed himself at Gibraltar, and saw enough of the eclipse in its partial stages (the total phase had no scientific interest for him) to secure what observations he desired. But he was baulked in another way. His measurements would be of no use without a very exact knowledge of the longitude of his station from a fixed observatory, or from Greenwich; without this, an essential datum, the astronomical time at which each was taken could not be obtained. Preparations of somewhat elaborate character were made to determine this longitude by the method of

little value; and they are quite useless where the longitude of the observing station is not very exactly known, as is often the case.

exchanging accurate time-signals by electric telegraph : Greenwich was to give its time to Gibraltar, and Gibraltar to return its time to Greenwich; Gibraltar local time being accurately determined at Professor Newcomb's temporary observatory. This exchange was to be made on several days before and after the eclipse through the medium of the Falmouth and Gibraltar cable, but, as may be remembered, the cable broke early in December, and it was not repaired till long after Professor Newcomb had left the Rock. His observations are consequently useless until another opportunity offers for effecting the longitude determination : then they can be made available.

The eclipse has therefore been of some import to metrical astronomy. Let us now take a glance at the materials which have been gleaned from it towards a solution of the physical questions at issue at the time of its occurrence, and to which it was appealed to decide. Of the phenomena revealed when the moon hides the photosphere of the sun, the *corona* only remains enigmatical. Baily's heads were long ago explained out of interest, and the red prominences now no longer need an eclipse to bring them under study. The object the most striking and the most anciently remarked * is still the most bewildering. At the time of the eclipse four modes of observation were at hand to resolve the mystery of its nature. First, eye-sketches, with or without telescopic aid, to decide whether the corona is similarly depicted by observers near together and far apart. Second, photographic pictures, which would give the aspect of the corona free from personality (though they may include subjective appearances of photo-chemical character). Third, spectroscopy, to determine the gaseous or incandescent solid condition of the *original source* of the coronal light. Fourth, polariscopy, which it was hoped would show whether that source is in the corona itself or apart from

* Dr. Schmidt, of Athens, calls attention to the following account of an eclipse seen at Corfu in A.D. 968, in which the corona is very clearly described :—"Leon, the deacon, reports thus concerning the eclipse. 'The appearance of the eclipse was of this nature : December was carrying on its 22nd day, and in the fourth hour of the day, the sky being clear, darkness covered the earth, and the brightest stars appeared; and it was possible to see the disc of the sun obscure and without brightness, but with a certain radiance, faint and pale, in the manner of a fine band shining in a circle round the disc along its outer edge : and the sun, overlapping the moon a little (for she appeared directly intercepting him), sent out its own rays and filled the earth with light.'" The date curiously coincides with that of the last eclipse, though it is Old Style reckoning. The appearance of the stars seems to prove that the eclipse was total : it is marked so in the reliable list of Eclipses given in the *Art de Vérifier les Dates des Faits Historiques.*

it; in other words, whether the coronal glow shines by its own or by reflected light.

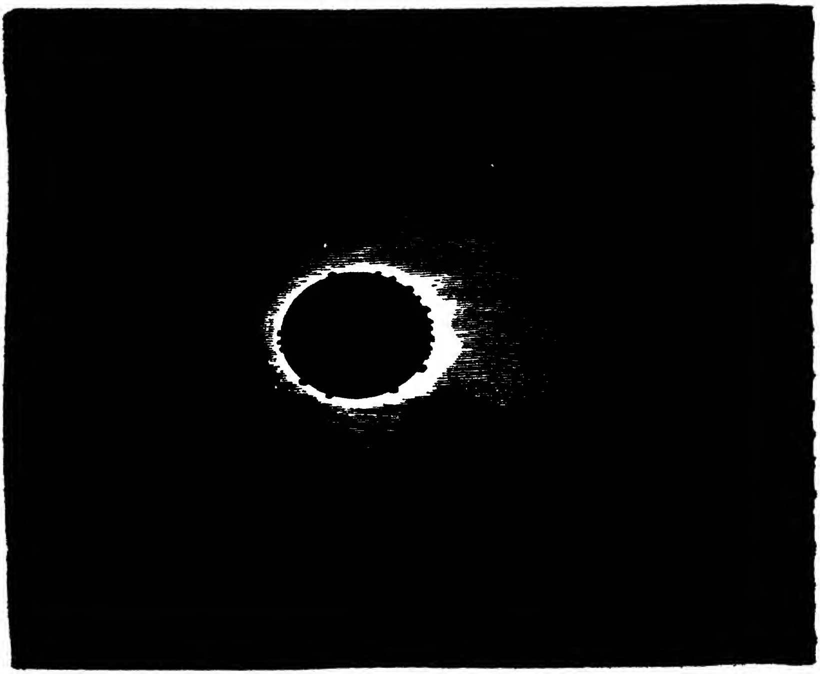
We will briefly review the results obtained by each of these methods. And, first, with regard to the eye-sketches.

A fair number of these, all made, however, at the Spanish end of the shadow-path, have come under our notice, some by artists of pretension, others by draughtsmen of no pretension: and very different are the impressions which these various drawings convey. Clearly, the corona has made greatly varying images upon retinæ of different eyes, or, what is perhaps more likely, the draughtsmen have considerably varied in their powers of conveying their impressions to paper.* Some drawings show a tolerably uniform circle of light, fading equably into the surrounding darkness. Such pictures admit of little inference being drawn from them. Others, on the contrary, exhibit definite points of structure, and where these are apparent in depictions of different observers, we can scarcely doubt the reality of their existence. The tendency of the corona to a roughly quadrangular contour is one feature thus certified; the existence of a definite zone of bright light (which observers of previous eclipses have noted) is another; and it has been suggested that this be named the *leucosphere.* But perhaps the most remarkable appearance is that of a V-shaped rift in the south-eastern part of the broad coronal haze; and as this is exhibited in several of the drawings, there can be no doubt about its real existence in the corona itself. Another notable feature is presented in three drawings, made by an American observer in Spain, one at the beginning of totality, another in the middle, and a third near the end of totality. In the first of these we see the greatest width of coronal light on the advancing side of the moon; in the second an equal width all around the moon; and in the third an excess on the following side of the moon; the whole series suggesting that the moon acted like a moving shutter intercepting the back-light first

* It is probable that much difference may arise from the mere materials of drawing and the fitness of these for the subject. To reproduce a hazy object like the corona, the worst thing to commence with is a sheet of white paper, even if it has a black disc printed upon it to represent the moon. Upon such a tablet great difficulty will arise in working the black sky with the requisite softness around the outer indefinite boundary of the hazy circle, especially if structural details have at the same time to be exhibited. It is in struggling against this difficulty with diverse materials—pencil, chalk, water-colour—that such strangely dissimilar effects are produced. The best basis for reproducing the corona rapidly and effectively would be a sheet of dark grey paper, with a black circle for the moon's disc; and the drawing material should be white chalk, which, upon the grey ground, will produce the desired effects at once with any degree of softness or decision.

on one side, then on the other, from some reflective matter in front of the shutter. This is the *primâ facie* idea conveyed; it is not to be taken as a proffered explanation of the appearance, though it agrees with Oudemans' theory of the coronal beams. Such a transfer of coronal light from one side of the black moon to the other has been depicted before, notably by Professor Plantamour, in his drawings of the eclipse of 1860, July 18, and it led the Astronomer Royal to suggest a cause with which Oudemans' theory is in accordance.

Of the photographs secured, three have been exhibited as



COPY OF MR. BROTHERS' TOTALITY PHOTOGRAPH.*

bearing upon the questions at issue—one taken by Lord Lindsay, in Spain, at the commencement of totality, which is remarkable as exhibiting an excess of corona on the advancing side of the moon, and therefore corroborating the American observer's drawing above noticed; a second obtained by the American party at Xeres, and a third secured by Mr. Brothers at Syracuse. Now, these two last-mentioned photographs are

* From a woodcut lent by Messrs. Macmillan. The top represents the north. In viewing this picture regard must be had to the impossibility of reproducing in a woodcut the softness of a photograph.

the most important achievements of the whole eclipse operations. The American one was taken with a six-inch object-glass corrected for actinic rays; the Syracuse one with a photographic copying lens, four inches in diameter, of course mounted equatorially. In the latter there is an extent of corona wider than any other photograph has shown, wider even than that in the corresponding American photograph. No doubt this extension is due to the smallness and consequent brightness of the image, for the exposure was only eight seconds, while the American plate was exposed a minute and a half.* Taken alone, Mr. Brothers' picture is exceedingly valuable: upon one side, for about 120 degrees of the moon's circumference, it shows a spreading of the coronal light to two diameters of the moon; in the south-east part there is the conspicuous rift alluded to as shown by the drawings made in Spain; and there are two other less conspicuous rifts, one about 40 degrees (measured on the moon's circumference) upwards towards the east of the great one, and the other about 60 degrees away to the south. The bright leucosphere is plainly indicated, and a tendency to streaming rays not perfectly radial is decidedly perceptible, even in an enlarged and therefore depreciated copy. But the great value of this picture comes out when it is magnified to equal size and compared with the American one taken at Xeres. Then it is seen that in the main features, especially the conspicuous rifts, the two are *almost identical.* Slight differences can be made out in the positions of some rather indefinitely-marked portions of the coronal boundary, but on the whole the agreement is wonderfully close. Now, when it is remembered that these pictures were taken at points on the earth's surface 1,100 miles apart, and that one was taken in absolute time 45 minutes after the other, it is perfectly clear that so much of the corona as is common to the two photographs is cosmical and not atmospheric. This is a great point established. And yet those self-drawn portraits of the corona open out wide fields for speculation. What are we to infer from the wedge-shaped rifts? They seem to deny the possibility of the outer corona being anything like a cosmical cloud near to the moon, on either side, for in that case the moon's motion should have affected them. If they were caused by the shadows of lunar mountains cast upon a sub-lunar mist, in accordance with Oudemans' theory, the motion aforesaid ought to have modified them considerably

* In the American picture there is some appearance of the outer coronal light having been cut off as by a diaphragm in the telescope. Structural details are reported to exist in the original negative, which, from causes that will be obvious to a photographer, are not reproduced in an enlarged copy such as that which we have before us.

during the long exposure of the American negative, and the photographed effects of their alteration would have made that picture differ greatly from the one obtained by Mr. Brothers, which had a very brief exposure. And their appearance is incompatible with the supposition of a cosmical cloud near to, though not connected with, the sun; for in that case, why should the rifts maintain a nearly radial position? And if we conceive the corona to be a solar envelope, we cannot regard the gaps as conical openings therein: they must be looked upon as valleys of great extent in the direction of our line of sight. They rather indicate vacant spaces between groups of radiant streamers of luminous or illuminated matter; and this interpretation may well be put upon them by those who have been arguing upon the analogy of the corona to terrestrial *auroræ* and its possible connection therewith—a fascinating subject upon which one would be disposed to dwell if the identity of the coronal with the auroral spectrum lines were less doubtfully established.

The spectroscopic results from the eclipse are tolerably numerous. The chief point of interest in them is the ample verification of the existence of the green (iron?) line corresponding to "1474" of Kirchoff's scale, not only in the spectrum of the leucosphere, but in the far outlying regions of the corona. Professor Harkness, at Syracuse, saw the line in all parts as far as 10′ from the moon (or sun), and suspected two other green lines less refrangible. Mr. Burton, at Augusta, saw the line also. Professor Winlock, at Xeres, saw it everywhere for 20′, or two-thirds the solar diameter, around the sun. Professor Young, also at Xeres, found it half a diameter off. Carpmael, at Estepona, saw three lines, one of which is doubtless the "1474;" we may well infer the same of one of two lines seen by Professor Denza, in Sicily. And we can scarcely doubt that one of those seen by Captain Maclear at San Antonio was also the now famous line; and if so it was, with others (C, D, and F), seen faintly *on the moon's disc*, thus clearly indicating reflection in our own atmosphere or in some medium between us and the moon. It is thus rendered almost certain that some of the distant coronal haze is reflected light, and this view is strengthened by the facts that Harkness saw a complete hydrogen spectrum when no prominence was near his slit, and that Young saw the C-line far above any possible hydrogen atmosphere.

Enough of observations have probably been secured to fix indubitably the position of the "1474" line. If more information is needed, it would be worth while to try if it cannot be procured from the moon. A spectator upon our satellite at sunrise would see the corona peep above his horizon some

time before the appearance of the actual limb of the sun, and at sunset he would see the corona linger after the sinking of the solar disc. The lunar surface must at sunrise and sunset be illuminated by a *coronal twilight*, which will be of considerable duration on account of the moon's slow rotation. It is therefore possible that if the faint light seen upon the moon's terminator (the boundary zone of light and darkness) were analysed by the spectroscope it would reveal the coronal lines. At all events, the experiment would be worth trying, and there are abundant opportunities for it. The position of the "1474" line, with respect to lines of known substances, may thus be deliberately determined, and some more reliable evidence obtained to aid a judgment whether it belongs to a new element —as Professor Young has suggested, some occluded gas, perhaps standing in relation to the magnetic powers of iron of whose spectrum the line is apparently a part—or whether it may be due to iron in any uncommon form or condition.

Reverting to the immediate results of the late eclipse, we remark that a faint continuous spectrum of the corona, without visible dark lines, was noted by several observers, among whom were Lieutenant Brown, Captain Maclear, and Professor Winlock, and that a highly interesting observation, not relating to the corona, however, was made by Professor Young. At the commencement of the totality he saw for an instant the whole of the Fraünhofer lines of the solar spectrum reversed, and the field of his spectroscope *filled with bright lines*. He must then have caught a glimpse of the stratum of burning elements that lays immediately above the photosphere—an observation made once, and once only, by Lockyer, without an eclipse.

The polariscopic observations confirm those with the spectroscope which indicate that a part of the coronal light is reflected, though they leave open the question whether that reflection occurs in or beyond our atmosphere. Professors Pickering and Langley are reported to have found that a considerable proportion of the light is polarised, and in a radial direction; the first-named observer obtaining the same results with three forms of polariscope. Professor Blaserna, observing in Sicily, asserts that the corona was strongly polarised, the only doubt with him being as to whether the plane was radial to the sun or tangential. Mr. Ranyard, at Villamonda, made three observations, two of which showed what was expected to be observed in the case of radial polarisation. Mr. Pierce, jun., arrived at a similar result, and so did Mr. Ladd. Mr. Samuelson, observing not upon the corona but upon the sky, first, far on one side of the sun, and then far below it, found vertical polarisation.

It would be difficult at the present time to define the precise

points upon which our knowledge of the sun and its surroundings has been advanced by this particular eclipse. It has been rendered tolerably certain that there is around the sun a self-luminous shell (the leucosphere), extending upon an average a sixth of his diameter beyond the hydrogen atmosphere, the principal constituent of which shell is that unascertained matter which gives the "1474" line; and that although this shell is self-luminous, it yet reflects some light from the brilliant strata beneath it. In the second place, it appears probable that there is a great extension, irregular in character and with a tendency to radiality, of matter which has either self-luminosity of the same kind as the leucosphere, though more feeble, or that has a special aptitude for reflecting leucospheric light. In the third place, it is pretty certain that there is a considerable scattering of all light not intercepted by the moon in and by some medium on this side of that body—either our atmosphere or a cosmical haze. This is as much as can be safely said upon the strength of the evidence now before us. More may possibly be inferred when a searching examination of the complete and detailed observations (which the Astronomer Royal has suggested should be made in connection with the undigested observations of the 1860 eclipse) is accomplished. It is much to be desired that this should be done before the time of the next eclipse—the 11th of the coming December—as it is not improbable that points of inquiry may be raised which that eclipse, from its peculiar circumstances, will afford special opportunities for deciding. The shadow will pass over the lofty Neilgherry Hills of India, and most valuable observations, bearing upon the question of the corona's atmospheric constituent, may be made at the exceptional elevations thus accessible.* The duration of totality there will be a few seconds over two minutes. In Northern Australia, however, where the shadow passes over Arnhem Land, the totality will last four minutes, and we hear with satisfaction that one observer, M. Bulard, of the Algiers observatory, intends to station himself there, most probably with photographic apparatus. And we may well rely upon the energy of the Australian astronomers to make the most of the occasion.

* Considering how the balloon has been pressed into scientific service, one cannot help wishing that a high balloon view of a total eclipse could be obtained; but there is no chance of an opportunity for such a view soon occurring.

# GRAFTING; ITS CONSEQUENCES AND EFFECTS.

By MAXWELL T. MASTERS, M.D., F.R.S.

[PLATE LXXI.]

ANYONE who would write the history of grafting might readily fill a volume—a large one, and one as interesting as large. If he entered into technical details a great many volumes would be required. All that we have space to do here is to show that our forefathers were not ignorant of the practice, that the surgeons adopted it from the gardeners, that John Hunter made it the subject of experiment, and that in these days both surgeons and gardeners seem disposed to avail themselves yet more and more of the advantages it holds out. If we could induce any reader of a practical turn of mind, and a bent towards physiological enquiry, to turn his attention to the subject, we should be glad; for although among gardeners especially great use is made of the grafting process, it is perfectly clear that a vast field remains yet for research—research, too, almost certain to yield profitable results alike to science and to practice.

Though so largely practised by nurserymen, it is really doubtful if we know much more about the matter than did the "Scriptores Rei Rusticæ." Columella knew how to bud roses; he describes as many modes of grafting the vine as Beau Brummel had fashions for adjusting his necktie, while Virgil described the results with a neatness of expression that leaves only one regret—that the matter of his verse is less correct than the metre. It is the fashion to laugh at these old cultivators, who could wield the pen with as great facility as the pruning-hook, because their ideas of what could be done by means of grafting do not coincide with our own; but we should not be much surprised if in the future it turned out that the statements we have been accustomed to ridicule contain, nevertheless, much more of truth than is admitted at present. We do not venture to look forward to the time when apples shall grow on plane-trees, or ashen boughs enwreath themselves

in a white mantle of pear-blossom,* or when hogs shall crunch acorns that have fallen from the overhanging elm. Possibly none of these things will come to pass, and yet others equally strange have happened, as we shall endeavour to show by and by, while much at least of what the old writers tell us is literally true. In hundreds of nurseries at this season pears are being grafted on quince stocks, apricots on plums, apples on crabs, so that Virgil's statement,

> "Nec longum tempus et ingens
> Exiit ad cœlum ramis felicibus arbos
> Miraturque novas frondes et non sua poma,"

is as much a matter of fact as that if we commit a ripe seed to the ground under favourable conditions it will spring up in due season.

Who first among surgeons adopted the grafting process we do not know. Tagliacozzi (*Latinè* Taliacotius), who died in 1553, is the one most held in remembrance for his feats in requisitioning a portion of the skin of a bystander in order to supply the deficient organism of his patient. How this was done is told in language more expressive than polite by one Butler, and it may perhaps be said with justice that the "learned Taliacotius" owes his reputation among posterity more to the rhymes of Hudibras than to his own publications. John Hunter, who left very little unheeded as unworthy his attention, illustrated the grafting process by divers experiments, among which the most striking is perhaps the removal of the spur of a cock, and its successful implantation on to the comb. Hunter, too, practised a method of curing ulcers which has been revived within the last year or two by French surgeons, and carried out with much success in several of our own hospitals. The operation simply consists in the removal of minute pieces of healthy skin, and in their transfer to the diseased surface. Under fitting conditions, and with due precautions, adhesion takes place, the ulcer heals over, and what is usually a long and intractable sore is by these means rapidly and effectually cured.

We do not propose in this paper to enter at any further length into the historical or chirurgical portion of the subject. Our intention is simply to treat it from a physiological point of view, and to allude to certain facts or allegations which, if confirmed, will be of no small importance scientifically and practically.

* There is only a difference of one letter between the Greek words *μελία* = ash, and *μηλία* = pear. Is it possible that Virgil, recalling what some Greek friends had told him, or what he had read in some Greek author, confused the ash and the pear? This is hardly likely, and would not account for the other anomalous cases of grafting; nevertheless, the similarity is suggestive.

Plate LXXI

Before adverting to the artificial process as practised by the gardeners, it may be well to allude to what Nature herself does in this way without assistance from man. The union of branch to branch of the same tree is so common a phenomenon that we need not dwell upon it further than to note it as the simplest and commonest case of grafting, at least so far as flowering plants are concerned. Among the fungi, indeed, or even in the early stages of growth of the mosses, the young plants become so inextricably intergrafted that the so-called individual is really a republic one and undivided. In the higher plants the grafting process is exceptional, and is the result of some abrasion which removes the outer rind, and thus allows the growing tissues of the two abraded surfaces to come into contact, and under favourable circumstances to adhere to each other. Union of the contiguous branches of two trees of the same species is of equally common occurrence with that just mentioned, and to this occurrence the great size of some trees is attributable.

We mention these more familiar illustrations with nothing more than passing comment. They illustrate the power that growing vegetable tissues have of uniting, and that is all we want with their testimony in this place. More important for our purpose is the evidence that plants of different species will unite together. This has been denied, but there are plenty of cases on record, and one facetious observer (Charles Waterton) compared the union of a spruce-fir with an elm, and the consequent stunting of both, to the incongruous union of Church and State! Such cases are certainly abnormal and exceptional, but they exist nevertheless, as a visit to Richmond Park will attest. There may be seen, or might have been a year or two since, a thorn (*Cratœgus*) adherent to a horn-beam (*Carpinus*). There are cases where the contact of the two trees has been so firm and so persistent that at length the two have become actually inseparable unless great force were used. It must, however, be remembered that we cite these cases simply as instances of the union of two distinct species, not of grafting properly so called. The difference is this—a graft derives its nourishment through the stock on which it is placed, while in the cases just alluded to each plant, though firmly joined to its neighbour, is perfectly independent of it in the matter of food. The same statement, however, cannot be made with reference to the mistletoe or the *Loranthus*. These are different enough from the trees on which they grow; they adhere to their foster-parents with a tenacity greater than that of any graft, and they suck the very life-blood out of them, ensuring their own destruction by causing the death of the trees on which they grow. It is worth while noting this fact in connection with the well-known tendency that grafting, as artificially practised,

has of shortening the term of life of the plant. Other cases of natural union are worthy of remark, especially the union that sometimes takes place in roots. For many years it has been known that the stumps of silver-firs increased in diameter after the trunks had been felled. Here was a pretty case for those who held the presence of leaves as an essential to the due formation of wood. How would they get over this difficulty—that wood there was, and yearly increasing, and yet no leaves? Even quite recently one of our agricultural societies has awarded its prize to an essay in which the phenomenon in question is in some way or another explained by the antiseptic action of peat! What a delightful discovery! Would that the salt-beef in the brine-tub would increase in like manner! Jesting apart, the cause of the annual growth of the stumps of the silver-fir was satisfactorily shown some twenty years ago by the German botanist Goeppert.* He was enabled to prove that the roots of the felled tree inoculated with those of adjacent trees, and that a communication of the nutrient fluids from the sound tree served to keep life in the maimed one. Doubtless a similar root-union exists in other cases, and affords the explanation of the formation of those seemingly detached knobs of oak that one occasionally meets with.

Another instance of root-union is worth mention, not only for its inherent singularity, but because it will yield us important evidence by and by. We allude to the case of the red and white carrot recorded by Lindley. The two roots by some means became twisted one around the other and firmly united together. But this was not all. While the tops or crowns of the two carrots preserved their natural appearance above the point of union, it was very different below. In fact the characteristics of the roots below the union were exactly transposed. What should have been a red root became white, while the white root blushed with a redness not its own. We may illustrate what happened in the case of these carrots by the letter X, consisting as it does of two lines, one thick the other thin, crossing in the centre. Now, suppose the thick line to become thin below the junction, and the thin line to become thick, and we shall have a change analogous to that which took place in the carrots aforesaid.

Another curious phenomenon occasionally met with is the union of embryo to embryo, either within the seed or immediately after germination. In most cases a seed contains but one embryo plant, but there is always a provision made for more than one, and in fact sometimes two or more are produced, as in the orange (*Citrus*). The mistletoe is one of these

* "Ann. Sc. Naturelles," xix. 1843, p. 181, t. iv.

plants, apt to produce twin embryos, and, what is more to our point, the twain are not unfrequently adherent like their famous Siamese counterparts. We have before us as we write, thanks to the courtesy of an American correspondent, a case wherein two seedling plants of the Osage orange (*Maclura*) are thus united together. In this plant the seedling consists of a root or radicle, surmounted by a "caulicle" which bears the two seed leaves above which the stem proper begins. Now, in our specimen, the roots are free and the stems are free, but the two caulicles are intimately united throughout their entire length. In America, where the Osage orange is largely grown as a hedge-plant, such unions are said to be not infrequent. Mr. Thwaites, the eminent director of the Botanic Gardens, Ceylon, records * a yet more curious instance, wherein two embryos were contained in one seed of a fuchsia, the two embryos possessing, moreover, different characteristics—a circumstance probably due to their hybrid origin, the seed in question having been the result of the fertilisation of one variety of fuchsia by the pollen of another.

It would be easy to multiply instances, but we have said enough to show that union may, and does occasionally, take place between different parts of the same individual plants, or between different plants of the same species, and even between plants of different specific nature.

Gardeners have not been slow to avail themselves of this hint. At this season of the year, in our large nurseries, a small army of expert workmen may be seen preparing the stocks for the reception of the "graft," adjusting the latter in its place, and with an amount of precision, dexterity, and rapidity truly marvellous, the more so as a glance at the horny hands of the operators would not lead one to credit their owners with the possession of the requisite surgical nicety of manipulation. One main object of this grafting process is the multiplication of desirable varieties of fruit or other trees, which could not be reproduced by other means with sufficient certainty and rapidity, and in some cases not at all. Other reasons why grafting is done will become apparent as we proceed. In the meantime, we may briefly allude to some of the conditions for successful grafting, so far, at least, as they are yet known to us. The first is that the plants furnishing the stock and the scion respectively should be nearly related one to the other. We may set aside as fables the stories previously alluded to, or at any rate we may explain them by the operation of causes other than those of grafting properly so called. But there is something more than mere botanical kinship necessary, and what

* "Ann. Mag. Nat. Hist.," March 1848.

that is is at present in great degree a mystery. It is readily intelligible that there must be a certain conformity of habit between stock and scion, that the two must be well matched as regards vigour, health, time of starting into growth, and the like, that the tissues of the plant must be sufficiently alike to permit of due contact and union, and so on. But these facts will not suffice to explain the sympathies and antipathies which plants manifest. A pear (*Pyrus*) will graft on another pear, on a quince (*Cydonia*), or on a hawthorn (*Cratægus*); but there is difficulty in getting it to grow on an apple, and a like difficulty in inducing an apple to grow on a pear, closely as the two are related.

Cultivators are often sadly puzzled to find a suitable stock on which to "work," as they phrase it, some desirable variety, and it is only by repeated trials with various plants that they succeed. In such cases they have nothing to guide them but the general principle that there must be some near botanical affinity, and, as we have just seen, even that fails them occasionally. For years it was a hard matter to find a stock on which *Viburnum macrocephalum* could be grafted, in spite of there being plenty of near relations at hand. On the other hand, the Loquat (*Eriobotrya*) will graft on the pear, the *Eriostemon* on the *Correa*, genera which, under the circumstances, we should not call very closely allied, while, in numerous instances, evergreen plants will graft on stocks of deciduous plants. A perennial species of convolvulus grafted on an annual species has caused the latter to assume the perennial habit of the scion—nay, some French nurserymen have even succeeded in engrafting a bud *on a leaf*. Not only did union take place, but the leaf thus made to serve as a stock instead of speedily perishing, as it would have done under ordinary circumstances, acquired a greater degree of permanence—assumed, in fact, the characters of a stem.*

It is evident, then, that much yet remains to be learnt as to the why and wherefore of these sympathies and antipathies.

In addition to a certain not remote botanical affinity, and to conformity of physiological conditions, it is obvious that nice adjustment and accurate contact of the growing tissues must be secured and maintained if the graft is to be satisfactory.

> "On each lopp'd shoot a foster scion bind:
> Pith pressed to pith and rind applied to rind;
> So shall the trunk with loftier crest ascend,
> Nurse the new bud, admire the leaves unknown,
> And, blushing, bend with fruitage not its own."

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* "Gardeners' Chronicle," 1866, p. 386.

A close paraphrase, on the part of Erasmus Darwin, so far as the last lines are concerned, of those of Virgil, already cited.

Turning now to the effects produced by grafting on the scion and on the stock respectively, we open up a very interesting subject for enquiry, and we make apparent the objects for which grafting is employed. Gardeners, as a rule, hold that in the great majority of instances no effect beyond adhesion is produced. There are some plausible reasons for this opinion, it must be admitted, inasmuch as the change is very often not obvious on the surface. One experimenter tells us for instance that he grafted at various times on the same jargonelle pear no less than eighteen different grafts. Of these eighteen, ten were apples of various kinds, while the remainder were made up of pears, hawthorns, medlar, and quince. All these grafts, we are told, succeeded—at least for a time; fruit was produced from the scions in nearly all cases; but there is no evidence to show that this extraordinarily composite tree ever produced any fruit differing from the usual character of that naturally yielded by itself, or by its numerous parasitically attached grafts. More extraordinary still is a case wherein a French experimenter, M. Carillet, of Vincennes, first of all took two pear-trees, both of which were grafted on the quince stocks. These we will call A and B. A was planted in the usual way, then B was grafted on it, but in an inverted position, head downwards, roots uppermost. When the operation was completed, there were thus two pear-trees united by their leading shoots, but the upper one, B, was reversed in position, with its roots completely exposed. To add to the strangeness of the experiment, M. Carillet next grafted on the ends of four of the principal roots of B—quince-roots of course—four different varieties of pears, two of which succeeded; so that the entire plant consisted, first of a quince stock rooted in the soil and bearing a grafted pear; on this latter, but in an inverted position, was another pear, also grafted on the quince and with its roots uppermost; on these again were grafted two more pears. This illustration shows that the current of the sap is quite independent of the direction of the tissues. It must have passed as readily through the inverted as through the erect stem; but what is more to the point, so far as we are concerned at present, is that though the sap passed through no less than six different organisms, adherent one to the other, yet each portion of the composite structure retained its own individuality.*

There are, indeed, many cases in which no apparent change takes place, as a result of grafting. Archbishop Whateley, with a view of ascertaining whether any change

* "Gardeners' Chronicle," 1867, p. 947, ex "Revue Horticole."

would be produced in the time at which the leaves were unfolded, grafted a scion of an early variety of hawthorn on a late one, and *vice versâ*; but he found in both cases that the scions produced their leaves early or late, as the case might be, wholly irrespective of the habit of the stock on which they were grafted. In spite of this and many similar instances which might be cited, we imagine that when gardeners state that no change results from the grafting process they would convey the idea that the change is physiological rather than morphological—that the size, fruitfulness, flavour, period of ripening, and the like may be altered by grafting, but not the form. How far this is true we now proceed to show.

And first as to the effect of the stock on the scion:—In many cases no further effect is produced than the mere hastening of the period of flowering, but in other instances we have alterations in "habit," constitution, &c. Thus, if a stock be hardy enough to resist the inclemency of our winters, while the plant from which the scion is taken is tender, we may make the scion nearly as hardy as its foster nurse by grafting it on a hardy stock. Thus seedling plants of *Cupressus macrocarpa* have perished in the severe winter, while grafts on the same plant "worked on" the red-cedar *Juniperus virginiana* survived.

Habit:—By this expression gardeners and botanists mean the general aspect and appearance of a plant dependent on size, the way in which the branches come off, their direction, &c. There are good habits and bad habits in a cultural sense. If a plant be tender, it is, for some purposes at least, of a bad habit. If it grow away to a great size, and produce flower and fruit but scantily and at long intervals, a fruit-grower would condemn it as of a bad habit, while a timber merchant might look on it with a more favouring glance. For fruit-growing purposes it is usually desirable to secure a plant of comparatively dwarf stature and of prolific habit, and this, by a proper selection of "stocks," the gardener is enabled to do. He can convert a giant into a pigmy; he can in a short space of time cover the barren branches with fruit-buds in place of leaf-buds; he can change the size, enhance the flavour, modify the form, and alter the time of production of the fruit. All this may be done, in certain cases, by the choice of a suitable stock and a due knowledge of and provision for local circumstances of climate, soil, exposure, &c.

In the case of the apple a dwarf habit, an earlier and more abundant production of fruit are ensured by grafting upon the "Paradise," a stock yielded by a peculiar dwarf-growing, surface-rooting variety of apple. Many, but not all, pears are similarly affected by being grafted on the quince.

Among conifers corresponding alterations of habit have been frequently noted. One of the most curious is that recorded by M. Carrière, in which a species of *Libocedrus* (Cupressineæ) grafted on *Saxe-Gothæa* (Podocarpeæ) became entirely altered in appearance. The branches, instead of forming an elongated pyramidal mass, were directed nearly at right angles, so as to form a depressed spherical head.

Very curious also are the phenomena exhibited by what is called double-grafting in England, or *greffe sur greffe* by the French. We mention them in this place as illustrative of the effect of the stock on the scion. It has already been mentioned incidentally that some pears will not graft readily on the quince, and, consequently, that mode of enhancing fertility could not be adopted were it not for the ingenious process of double-grafting, which is thus effected:—In the first place some free-growing pear is grafted on to the quince in the usual manner, and then the scion so obtained is, in its turn, grafted with a variety that will not unite readily with the quince in the first instance. In this indirect manner the quince stock is made to affect the scion, throw it into bloom more quickly, and enhance its fertility.

It is needless after this to say one word more as to the reasons which lead gardeners to employ grafting; the only wonder is that they do not avail themselves of it more freely.

Adverting now to the effect produced by the scion on the stock, it may be said that we have here an operation something akin to vaccination; and it is not wonderful that curious results sometimes accrue, though in this case, also, many gardeners deny that any visible effect is perceptible, at least in the majority of cases. Some, however, more in the habit of making good use of their eyes, have recorded instances showing the effect of the scion on the stock, and to some of these we now propose to allude. The first case is that where an unhealthy or feeble stock has been restored to health by the imposition of a healthy graft. This fact is vouched for on the authority of the veteran fruit-grower and practical physiologist of Sawbridgeworth, Mr. Thomas Rivers. Again, it has been stated that if two quince stocks of equal strength and vigour be grown under the same conditions, and on the one be placed a graft of some vigorous growing pear, and on the other a scion of some weak variety, the stock in the first case will grow much more quickly than in the second; and, indeed, a quince stock on which a strong growing pear has been grafted may thus be made to produce within a given time a larger amount of wood than any ungrafted quince would do in the same time. Again, cases have been observed where from the stock *below* the graft fruits and flowers of the same appearance

as those borne on the scion have made their appearance. This has been observed in the case of the pear grafted on the mountain ash, and in other cases.

Variegated plants, however, afford the most striking illustrations of the effect of the scion on the stock. We will not here do more than allude to the classic case of the variegated jessamine at Chelsea, nor to other similar cases recorded in gardening books,* but confine ourselves to other more recent illustrations, most of which have come under our own observation. The first is a repetition of the jessamine case (already repeatedly confirmed), but presenting this peculiarity,—that the buds of the variegated scion only remained on the stock for a short time before they died. "Many years ago," says Mr. Godsall,† "I conceived that if the variegation of *Jasminum officinale* could be transferred to the *J. revolutum*, which has a larger and handsomer leaf, it would be desirable. I therefore budded plants of the latter with buds of the common variegated jessamine. The buds appeared plump for a time, and then all died off. Notwithstanding this, the following year the plants exhibited variegation in several leaves and shoots, continuing even along the young branches; but the variegation was white, whereas on the *J. revolutum* (which has yellow flowers) it became yellow. Last year I reversed the experiment, by budding the variegated *J. revolutum* on the plain *J. officinale*, and at the present time the yellow variegation appears on the leaves and young shoots." This change in colour in the variegation is a very singular circumstance, but one which the limits of this article do not permit us to dwell on.

The effect produced even by a temporary contact with the variegated bud is confirmed by a case that fell under our own observation. A year or two since a very beautiful *Abutilon*, with leaves mottled with yellow, was introduced into our gardens. It was very desirable that this should be propagated as largely and speedily as possible. Propagation by means of cuttings was easy enough, but naturally the plants were small, and took a considerable time to grow bigger. Grafting was therefore had recourse to. The scions of the variegated *Abutilon Thomsoni* were grafted on to green-leaved stocks of other *Abutilons*. This was done by many nurserymen on the Continent as in this country, and it was soon found that the grafted plants were apt to produce variegated leaves from the stock; in other words, that the peculiar qualities of the scion were manifested throughout the entire organism. We were indebted to Messrs. Downie,

* The reader will find several of these cases mentioned in Darwin's "Variations of Animals and Plants," vol. i. p. 394.

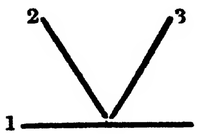
† "Gardeners' Chronicle," 1869, p. 838.

Laird, and Laing for the opportunity of examining a whole series of such plants thus changed. To show that the variegation was really due to the influence of the scion, we may mention a curious fact communicated to us by M. Van Houtte, the well-known nurseryman of Ghent. Like his compeers he had plenty of illustrations of the fact that a variegated scion of this particular *Abutilon* will communicate its properties to the stock on which it may be grafted, but he further ascertained that if by some accident the graft were separated from the stock, the leaves subsequently produced from the latter were wholly green, as before the grafting, and even the variegated leaves originally produced lost their mottled character. Our illustration shows a plant of the variegated *Abutilon* grafted on to another species of the same genus, which under ordinary circumstances produces green leaves, but which in consequence of the grafting has formed variegated leaves. Variegated willows have been known to affect the stock on which they are grafted in the same way, and also, but not constantly, the purple-leaved nut.

Here is another striking illustration of the effect of the scion on the stock. Two "canes" of a particular vine, called "Black Prince," were growing side by side in a vinery, and adjacent to them another grape known as "Lady Downe's Seedling." Mr. Smythe, who relates the case, "inarched" the last-named vine on to one of the canes of the "Black Prince," leaving the other cane untouched. A great change resulted in the appearance of the engrafted cane of the "Black Prince." In the first place it did not begin to grow so soon by fourteen days as its fellow cane; its leaves were smaller, its shoots shorter, and its bunches of fruit much shorter and smaller. These illustrations will probably be sufficient to show that the scion does affect the stock upon which it grows.

Relying on such cases as these some botanists have broached the theory of graft-hybridisation to account for certain anomalous appearances. If the pollen of one flower be applied to the stigma of another under certain conditions and within certain limits, a "cross" or hybrid production results, in which are blended in very varying degrees the characteristics of both parents. Now, it is contended by some that a similar blending takes place in the case of grafted plants, and that buds may occasionally be produced on such grafted plants partaking in varying degrees of the characters of both graft-parents. Others again deny the possibility of the occurrence, especially practical men, who, seeing in their daily practice that instances such as we have cited are exceptional, and that in the majority of cases stock and scion remain unaffected save in minor points, give a different interpretation to such facts as we have been commenting on, or content themselves with leaving them unexplained.

The case that has attracted most attention on the part of botanists and physiologists is that of the laburnum known in gardens as *Cytisus Adami*. This plant is stated to have originated in a French nursery from the insertion of a bud of the shrubby *Cytisus purpureus* on to the common *Cytisus Laburnum*. At any rate, great astonishment is excited year after year, as the tree produces the long racemes of the yellow-flowered laburnum and the shorter tufts of the purple *Cytisus*, while sometimes the colours of the flowers are mingled in the same raceme; and, indeed, every intermediate form is produced between the two species above enumerated. Those who dispute the origin of this tree from grafting attribute its peculiarities to the circumstance that it is a hybrid formed in the ordinary way by cross fertilisation, and that the two-faced appearance the plant puts on is simply the effect of the disunion of the mingled characteristics such as is now known to occur frequently among hybrid plants. But against this view there are certain circumstances to be recorded. First, the history of the plant, according to which it decidedly originated in a graft. The nurseryman could hardly have been mistaken in such a case, and there is no reason whatever to question his veracity. Secondly, attempts that have been made to produce the plant by pollen-fertilisation in the way suggested have not been successful. Were this a solitary case one might hesitate to accept it as a case of graft-hybridisation, but it is not unsupported by evidence of a similar character, as we have already shown. Even in the laburnum, Mr. Purser has produced a precisely similar effect by grafting the purple *Cytisus* on to the yellow-flowering-species. No doubts have ever been raised as to the correctness of this statement. We may further cite the case of a Devoniensis rose budded on a White Banksia, and wherein from immediately above the graft arose a branch which was neither White Banksian nor Devoniensis, but partook of the characters of both. This may be represented diagrammically, as in the figure:—1 represents the stock; 2 the graft, or bud; 3 the mixed product.




Other illustrations have been cited in roses, one of which is shown in the accompanying illustration(Plate LXXI.), and wherein a white moss-rose is shown springing from the same branch as a smooth-barked red Quatre-Saisons rose. The graft in this case was effected close to the ground while the two flowers were borne two feet above it. Those who disbelieve in the possibility of graft-hybridisation attribute such a case as this to the separation between the heretofore mixed elements of an ordinary hybrid.

Not long since a well-known French horticulturist, M. Carrière, put on record a case in which he grafted a scion of

*Sorbus nepalensis.* The buds which were subsequently produced were found to be different from those of the scion and of the stock (unfortunately it is not stated what plant furnished the latter) in hardiness, period of leafing, form, &c.; indeed, so different were the new productions, that they were, says M. Carrière, unlike any plant he knew in cultivation. This also may possibly have been a case of variation not necessarily connected with grafting, but it is too singular to be passed over in this place.

Of doubtful origin, also, are the oft-mentioned "trifacial oranges"—oranges in which the fruits presented, blended together in all possible proportions, the characteristics of two or three distinct varieties of orange. As the true nature of these singular fruits is shrouded in mystery, and we can add nothing to their history beyond what is detailed in readily accessible books, such as Lindley's "Theory of Horticulture," or Darwin's "Variations of Animals and Plants," we pass them by here with the mere mention.

The latest development of the graft-hybridisation theory is that according to which certain new or strange variations of the potato have been attributed to this process. The "eyes," or buds, of one kind of potato have been inserted into the tuber of another kind, carefully deprived of its own buds; adhesion has taken place, and new tubers have been formed, differing from those of the parent variety, and producing leaves and haulm also different in character, and to some extent intermediate. It is only right to add, in reference to this point, that the majority of experimenters have failed even in getting adhesion under such circumstances. Others deny the possibility of the occurrence *in toto*, and attribute any changes that may have occurred to the known variability and tendency to "sport" exhibited by the potato. So far as our own personal experience goes, we have seen several cases of adhesion in potatoes grafted by others; and have ourselves succeeded in obtaining union in one instance. Moreover, there has been abundant evidence to show that some change of an extraordinary character does take place in the new tubers that originate after grafting. Whether or no these changes are due to a commingling of characteristics derived from the stock and the scion is a matter still open to question.

It must not, however, be supposed that all cases of bud-variation are due to graft-hybridisation. The evidence in favour of the latter process is as yet by no means free from doubt. Nevertheless, confirmatory facts, or what, so far as we know at present, we have good reason to believe to be such, are gradually accumulating. At any rate, it seems clear that the old notion that graft and scion, scion and graft, have no reciprocal effect, must be given up as untenable.

The physiology of cell life does not at present help us very much in the elucidation of the effects produced by grafting. Those who deny that any effect is produced beyond adhesion of the graft to the stock, and the transmission of fluid from the roots through the latter to the scion, have anatomy in their favour. The tissue below the graft is that of the stock; above it, it is that of the graft—at least under ordinary circumstances. Moreover, it is well known that to a large extent each cell is independent of its neighbour, and often contains very different ingredients, without any intermingling of the contents of adjacent cells. This is well shown in the case of the red beet grafted on the white beet—the two retained perfectly their respective characteristics above and below the union—as well as by other illustrations previously cited. But, on the other hand, this fact should be compared with the transposition of characters presented by the two carrots alluded to in an earlier page. Those who lean to the view that stock does affect scion and scion stock have only exceptional aid from anatomy; but physiologically, they may avail themselves of the circumstance that the passage of fluids, and to some extent the direction of new growth, are now known not to be limited to any single course, but to take place in any direction, according to circumstances.

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## EXPLANATION OF PLATE LXXI.

*a* to *c*, Abutilon.

*a*. Stock of normally green-leaved Abutilon, on which at *b* is grafted a scion of the variegated Abutilon Thomsoni.

*c c* are branches of the stock, the leaves of which have become variegated in consequence of the grafting.

*d*, *e*, Roses.

*d*, a white moss-rose.

*e*, a red smooth-stemmed Quatre-Saisons rose, proceeding from the same branch as *e*, possibly as a result of grafting.

# COAL AS A RESERVOIR OF POWER.

By ROBERT HUNT, F.R.S.

THE sun, according to the philosophy of the day, is the great storehouse of Force. All the grand natural phenomena are directly dependent upon the influence of energies which are poured forth without intermission from the central star of our system. Under the influences of light, heat, actinism and electricity, plants and animals are produced, live and grow, in all their infinite variety. Those physical powers, or, as they were formerly called, those imponderable elements, have their origin in one or other of those mysterious zones which envelope the orb of day, and become evident to us only when mighty cyclones break them up into dark spots. Is it possible to account for the enormous amount of energy which is constantly being developed in the sun? This question may be answered by saying, that chemical changes of the most intense activity are discovered to be for ever progressing, and that to these changes we owe the development of all the physical powers with which we are acquainted. In our laboratory we establish, by mechanical disturbance, some chemical phenomenon, which becomes evident to our senses by the heat and light which are developed, and we find associated with them the principle which can set up chemical change and promote electrical manifestations. We have produced combustion, say, of a metal, or of a metallic compound, and we have a flame of a colour which belongs especially to the substance which is being consumed. We examine a ray of the light produced by that flame by passing it through a prism, and this analysis informs us that coloured bands, having a fixed angle of refraction, are constant for that especial metal. Beyond this, research acquaints us with the fact that, if the ray of light is made to pass through the vapour of the substance which gives colour to the flame, the lines of the spectrum which were chromatic become dark and colourless. We trap a ray of sunlight and we refract it by means of a spectroscope—an instrument giving

results which are already described in this journal *—when we detect the same lines as those which we have discovered in our artificial flame. We pursue this very interesting discovery, and we find that several metals which give colour to flame, and produce certain lines, when subjected to spectrum analysis, are to be detected in the rays of the sun. Therefore our inference is, that some substances, similar to the terrestrial bodies, with which we are familiar, are actually undergoing a change in the sun, analogous to those changes which we call combustion; and, more than this, we argue that the high probability is, that all solar energies are developed under those conditions of chemical change—that, in fact, the sun is burning, and while solar matter is changing its form, Force is rendered active, and as ray-power passes off into space as light, heat, &c. to do its work upon distant worlds, and these forms of Force are expended in doing the work of development *on* those worlds. This idea—theory—hypothesis—call it what we may—involves of necessity the waste of energy in the sun, and we must concede the possibility of the blazing sun's gigantic mass becoming eventually a globe of dead ashes, unless we can comprehend some method by which energy can be again restored to the inert matter. Certain it is that the sun has been shining thousands of years, and its influence on this earth we know to have been the production of organised masses, absorbing the radiant energies, in volumes capable of measurement. On this earth for every equivalent of heat developed, a fixed equivalent of matter has changed its form; and so likewise is it with regard to the other forces. On the sun, in like manner, every cubic mile of sunshine represents the change of form of an equivalent of solar matter, and that equivalent of matter is no longer capable of supplying Force, unless by some conditions beyond our grasp at present it takes up again that which it has lost. That something of this kind must take place is certain. The sun is *not burning out*. After the lapse of thousands of years we have the most incontrovertible evidence that the light of to-day is no less brilliant now, than it was when man walked amidst the groves of Eden. We may venture further back into the arcana of time, and say that the sun of the past summer has shone with splendour equal to the radiant power which myriads of ages ere yet man appeared on this planet stimulated the growth of those luxuriant forests which perished to form those vast beds from which we derive our coal. Not a ray the less is poured out in any hour of sunshine: not a grain weight of matter is lost from the mass of the sun. If either the sunshine was weakened, or the weight of the vast globe diminished, the planets would

* "Popular Science Review," vol. 1, pp. 210–214.

vary in their physical conditions, and their orbits would be changed. There is no evidence that either the one or the other has resulted. Let us see if we can guess at any process by which this stability of the solar system is maintained.

It was first shown by Faraday, in a series of experimental investigations which may be regarded as the most beautiful example of inductive science with which the world has been favoured since Bacon promulgated his new philosophy, that the quantity of electricity contained in a body, was exactly the quantity which was necessary to decompose that body. For example, in a voltaic battery—of zinc and copper-plates—a certain fixed quantity of electricity is eliminated by the oxydation of a portion of the zinc. If, to produce this effect, the oxygen of a given measure of water—say a drop—is necessary, the electricity developed will be exactly that which is required to separate the gaseous elements of a drop of water from each other. An equivalent of electricity is developed by the oxydation of an equivalent of zinc, and that electricity is required for the decomposition of an equivalent of water, or the same quantity of electricity would be equal to the power of effecting the recombination of oxygen and hydrogen, into an equivalent of water. The law which has been so perfectly established for electricity is found to be true of the other physical forces. By the combustion—which is a condition of oxydation—of an equivalent of carbon, or of any body susceptible of this change of state, exact volumes of light and heat are liberated. It is theoretically certain that these equivalents of light and heat are exactly the quantities necessary for the formation of the substance from which those energies have been derived. That which takes place in terrestrial phenomena is, it is highly probable, constantly taking place in solar phenomena. Chemical changes, or disturbances analogous to them, of vast energy are constantly progressing in the sun, and thus is maintained that unceasing outpour of sunshine, which gladdens the earth, and illumines all the planets of our system. Every solar ray is a bundle of powerful forces: light, the luminous life-maintaining energy, giving colour to all things; heat, the calorific power which determines the conditions of all terrestrial matter; actinism, peculiarly the force which produces all photographic phenomena; and electricity regulating the magnetic conditions of this globe. Combined in action, these solar radiations carry out the conditions necessary to animal and vegetable organization, in all their varieties, and *create* out of a chaotic mass forms of beauty rejoicing in life.

To confine our attention to the one subject before us. Every person knows that to grow a tree or a shrub healthfully, it must have plenty of sunshine. In the dark we may force a plant to

grow, but it forms no woody matter, it acquires no colour; even in shade it grows slowly and weak. In sunshine it glows with colour, and its frame is strengthened by the deposition of woody matter eliminated from the carbonic acid of the air in which it grows. A momentary digression will make one point here more clear. Men and animals live by consuming the products of the vegetable world. The process of supporting life by food is essentially one of combustion. The food is burnt in the system, developing that heat which is necessary for life, and the living animal rejects, with every expiration, the combinations, principally carbonic acid, which result from this combustion. This carbonic acid is inhaled by the plant; and, by its vital power, excited by sunshine, it is decomposed; the carbon forms the ligneous structure of the plant, and the oxygen is liberated to renew the healthful condition of the atmosphere. Here we see a sequence of changes analogous to those which have been shown to be a law of electricity.

Every equivalent of matter changing form in the sun sends forth a measured volume of sunshine, charged with the organising powers as potential energies. These meet with the terrestrial matter which has the function of living, and they expend themselves in the labour of producing a quantity of wood, which represents the equivalent of matter which has changed form in the sun. The light, heat, chemical and electrical power of the sunshine have produced a certain quantity of wood, and these physical energies have been absorbed—used up—in the production of that quantity. Now, we learn that a cube of wood is the result of a fixed measure of sunshine: common experience teaches us that if we ignite that wood it gives out in burning, light and heat; while a little examination proves the presence of actinism and electricity in its flame. Philosophy teaches us that the powers set free in the burning of that cube of wood are exactly those which were required for its growth, and that, for the production of it, a definite equivalent of matter changed its form on a globe ninety millions of miles distant from us.

Myriads of ages before man appeared—the monarch of this world—the sun was doing its work. Vast forests grew, as they now grow, especially in the widespread swamps of the tropics, and, decaying, gathered into thick mats of humus-like substance. Those who have studied all the conditions of a peat morass, will remember how the ligneous matter loses its woody structure in depth—depth here representing time—and how at the bottom a bituminous or coaly matter is not unfrequently formed. Some such process as this, continued through long ages, at length produced those extensive beds of coal which are so distinguishingly a feature of the British and American coal-

fields. At a period in geological time, when an Old Red Sandstone land was washed by ocean waves highly charged with carbonic acid, in which existed multitudinous animals, whose work in nature was to aid in the building up masses of limestone rock, there prevailed a teeming vegetation from which has been derived all the coal-beds of the British isles. Our space will not allow of any inquiry into the immensity of time required for the growth of the forests necessary for the production of even a single seam of coal. Suffice it to say, that within one coal-field, we may discover coal-beds to the depth of 6,000 feet from the present surface. The section of such a coal-field will show us coal and sandstone, or shale, alternating again and again—a yard or two of coal and hundreds of feet of shale or sandstone—until we come to the present surface; everyone of those deeply-buried coal-beds having been at one time a forest, growing under the full power of a brilliant sun, the result of solar forces, produced then, as now, by chemical phenomena taking place in the sun itself. Every cubic yard of coal in every coal-bed is the result of a very slow, but constant, change of a mass of vegetable matter; that change being analogous to the process of rotting in a large heap of succulent plants. The change has been so slow, and continued under a constantly increasing pressure, that but few of the gaseous constituents have escaped, and nearly all those physical forces which were used in the task of producing the woody matter of the plant, have been held prisoners in the vegetable matter which constitutes coal. How vast, then, must be the store of power which is preserved in the coal deposits of these islands!

We are now raising from our coal-pits nearly one hundred and ten millions of tons of coal annually. Of this quantity we are exporting to our Colonial possessions and Foreign parts about ten million tons, reserving nearly a million tons of coal for our home consumption. Not many less than one hundred thousand steam boilers are in constant use in these islands, producing steam,—to blow the blast for smelting the iron ore,—to urge the mills for rolling, crushing, and cutting with giant power,—to twirl the spindle,—and to urge the shuttle. For every purpose, from rolling cyclopean masses of metal into form to weaving silky textures of the most filmy fineness, steam is used, and this steam is an exact representative of the coal employed, a large allowance being made for the imperfections of human machinery. This requires a little explanation. Coal is a compound of carbon, hydrogen, oxygen, and nitrogen, the last two elements existing in quantities so small as compared with the carbon, that they may be rejected from our consideration. The heat which we obtain in burning the coal is almost all derived from the carbon; the hydrogen in burning produces some heat,

but for our purpose it is sufficient to confine attention to the carbon only.

One pound of pure coal yields, in combining with oxygen in combustion, *theoretically*, an energy equal to the power of lifting 10,808,000 pounds one foot high. The quantity of heat necessary to raise a pound of water one degree will raise 772 pounds one foot. A pound of coal burning should yield 14,000 units of heat, or $772 \times 14{,}000 = 10{,}808{,}000$ pounds, as above. Such is the theoretical value of a pound of pure coal. Many of our coal-seams are about a yard in thickness; several important seams are much thicker than this, and one well-known seam, the thick coal of South Staffordshire, is ten yards in thickness. This, however, concerns us no further than that it is useful in conveying to the mind some idea of the enormous reservoir of power which is buried in our coal formations. One square yard of the coal from a yard-thick seam—that is, in fact, a cubic yard of coal—weighs about 2,240 pounds avoirdupois; the reserved energy in that cube of coal is equal to lifting 1,729,200 pounds one foot high. We are raising every year about 110,000,000 tons of coal from our coal-beds, each ton of coal being about a square yard. The heat of that coal is equal to a mechanical lifting power which it is scarcely possible to convey to the mind in anything approaching to its reality. If we say it is 190,212,000 millions we merely state an incomprehensible number. We may do something more than this, if we can convey some idea of the magnitude of the mass of coal which is raised annually in these islands.

The diameter of this globe is 7,926 miles, or 13,880,760 yards; therefore the coal raised in 1870 would make a solid bar more than eight yards wide and one yard thick, which would pass from E to W through the earth at the equator. Supposing such a mass to be in a state of ignition, we can perhaps imagine the intensity of its heat, and its capability, if employed in converting water into steam, of exerting the vast force which we have endeavoured to indicate. It was intimated last year in the House of Commons by a member of the Coal Commission that the decision of that body, after a long and laborious inquiry, would be that there existed in our coal-fields a supply for about one thousand years at our present rate of consumption. We have therefore to multiply the above computation by 1,000 to arrive at any idea of the reserved power of our British coal-fields. What must it have been ere yet our coal deposits were disturbed! At the time of the Roman occupation coal was used in this country. In the ruins of Roman Uriconium coal has been found. Certainly up to the present time a quantity of not less than three thousand million tons of coal have been dug out of our carboniferous deposits and consumed. All this

enormous mass of matter has been derived from vegetable organizations which have been built up by sunshine. The sun-rays which compelled the plants to grow were used by the plant, absorbed, imprisoned in the cells, and held there as an essential ingredient of the woody matter. The heat, light, actinism, and electricity, which are developed when we burn a lump of coal, represent exactly the quantity of those forces which were necessary to the growth of the vegetable matter from which that coal was formed. The sunshine of infinitely remote ages becomes the useful power of the present day.

Let it not, however, be supposed that we employ all the heat which is available in our coal. All our appliances, even the very best, are so defective that we lose far more than we use. A pound of pure coal should evaporate thirteen pounds of water; in practice a pound of coal does not evaporate four pounds, even in the most perfectly constructed steam-boilers, with the most complete steam-engines, such as have been constructed for pumping water for the Chelsea and the other waterworks upon the Thames.

Numerous attempts have been made to burn our coal so as to secure a more effective result than this. There has been some advance, the most satisfactory being in the regenerative furnace of Mr. Siemens. In this system the solid fuel is converted into crude gas, this gas is mixed with a regulated quantity of atmospheric air, and then burnt. The arrangements are essentially *the gas producer*, or apparatus for converting the fuel bodily into a gaseous state; then there are *the regenerators*. These are sunk chambers filled with fire-bricks, piled in such a manner that a current of air or gas, passing through them, is broken into a great number of parts, and is checked at every step by the interposition of an additional surface of fire-brick; four of these chambers are placed below each furnace. The third essential is the *heated chamber*, or furnace proper. This, the furnace chamber, communicates at each extremity with two of the regenerative chambers, and, in directing currents of gas and air upwards through them, the two gaseous streams meet on entering the heated chamber, where they are ignited. The current descends through the remaining two regenerators, and heats the same, in such a manner, that the uppermost chequerwork is heated to nearly the temperature of the furnace, whereas the lower portions are heated to a less and less degree, the products of combustion escaping into the chimney comparatively cool. In the course of, say, one hour, the currents are reversed, and the cold air and gas ascending through the two chambers which have been previously heated, take up the heat there deposited, and enter into combustion at their entrance into the heated chamber, at nearly the tem-

perature at which the products of combustion left the chamber. It is not difficult to conceive that by this arrangement, and with its power of accumulation, any degree of temperature may be obtained in the furnace chamber, without having recourse to purified gas, or to an intensified draught. Where the temperature of the melting chamber has certainly exceeded 4,000 degrees of Fahrenheit, the products of combustion escape into the chimney at a temperature of only 240 degrees. The practical result of this regenerative system is stated to be, that a ton of steel requires by the ordinary method about three tons of Durham coke,—which, being estimated as coal, will be about four tons,—to melt it, whereas in Siemens's furnace, the melting is effected with twelve hundred-weight of ordinary coal. This economy is produced by reserving the heat, by means of the regenerator, which is ordinarily allowed to escape by the chimney.

Another plan for consuming coal with economy has been recently introduced by Mr. T. R. Crampton, and is now in use at the Royal Arsenal, Woolwich, and at the Bowling Iron Works, in Yorkshire. Instead of converting the coal into gas, as in the Siemens' process, the coal is reduced by Mr. Crampton to a very fine powder, and then blown into the heated chamber by means of a fan-blast. By this arrangement the perfect combustion of the coal is produced, and a heat of the highest intensity can be obtained. The utilisation of this heat, without waste, when it is produced, is an important question still requiring careful attention. There are several other experiments being carried out with a view to the economical use of coal, but the two to which we have alluded give up to the present time the best results. Still, with these we allow more than one half of the heat latent in the coal to escape us. The subtle element eludes our grasp—our charms are powerless to chain the sprite; he will not be bound to labour for us, but passes off into space, regardless of the human Prospero whose wand of science he derides.

In conclusion, our philosophy has enabled us to determine the heat-value of our coal-fields, and to prove that all this heat has a solar origin. Our science has shown us that although we can eliminate all this heat, we cannot use it. There is an immense quantity constantly passing into space as radiant heat which we cannot retain.

The circle of action between the vegetable and the animal world, is a beautiful and a remarkable provision. The animal burns carbon and sends into the air carbonic acid (a compound of carbon and oxygen), the vegetable breathes that carbonic acid and decomposes it, the carbon is retained and the oxygen liberated in purity, to maintain the life and fire-supporting principle of the atmosphere. Changes similar to these may be

constantly going forward in the sun, and producing those radiations which are poured forth in volumes, far beyond the requirements of all the planets of our system. Although there is probably some circle of action analogous to that which exists upon this earth, maintaining the permanency of the vegetable and animal world, still there must be a waste of energy, which must be re-supplied to the sun.

May it not be, that Sir Isaac Newton's idea,—that the comets traversing space gather up the waste heat of the solar system, and eventually, falling into the sun, restore its power,—is nearer the truth than the more modern hypothesis, that meteorites are incessantly raining an iron shower upon the solar surface, and by their mechanical impact reproducing the energy as constantly as it is expended.

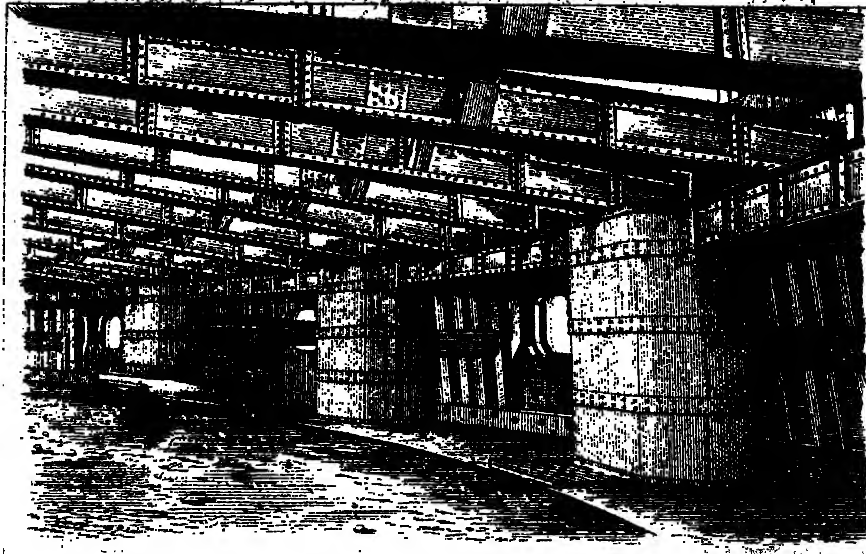
# THE PLYMOUTH BREAKWATER FORT.

By S. J. MACKIE, C.E.

[PLATE LXXII.]

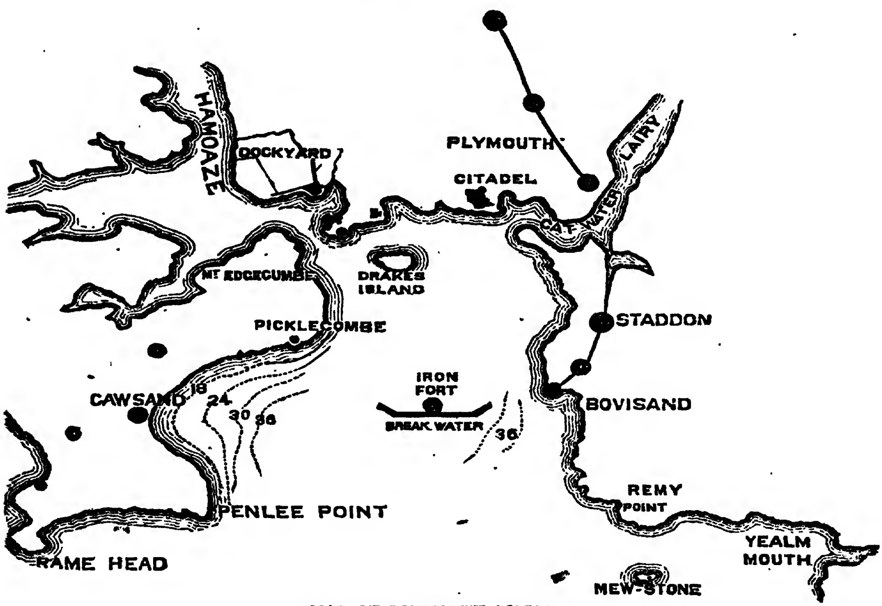
TO comprehend the nature and value of the Plymouth iron fort, its position, extent and nature should be clearly defined. Plymouth Sound is a vast open U-shaped natural harbour, a little over three miles in length, and about as wide at its mouth in a direct line between Penlee Point on the west, and Reny Point on the east. At about one-third of its area the turbulent waves which roll in from the Atlantic are checked by a great breakwater formed of rocks quarried from the neighbouring hills, mainly at Oreston. In length, the breakwater is about 5,000 feet long, rising up with very shelving slopes out of some six to seven fathoms of water (at low tide). On the eastern side of the Sound, Staddon Point juts out well towards the eastern arm of the breakwater; but on the western side the shore recedes between Penlee and Picklecombe into a large bay at Cawsand, forming a considerable expanse of water. The actual deep-draught shipways, of more than a thousand feet in width, pass on either hand close to the horns of the breakwater, nearly behind the centre of which, at a distance of a hundred yards, is placed the iron fort, to which this article is devoted. At Picklecombe and at Bovisand (the point of Staddon) there are granite casemated forts defending the water areas a little way in the rear; the distances of these batteries being each about a mile from it. A mile and a half up the Sound is Drake's Island, and half a mile further in the background the heights of Plymouth, also fortified, as well as are the entrances to the Hamoaze on the one hand, and to the Lairy on the other. The position of the breakwater fort is thus a most important one, and it is to be regarded as the main defence of the Sound.

The iron fort is based upon masonry foundations faced with granite, and rising 16 feet 6 inches above high-water spring tides. Its walls are set in from the masonry about 3 feet, there being thus a clear walk or glacis of that width all round



*Interior shewing Position of Guns.*

it. In form it is an oval, measuring upon its major axis at the base 143 feet 6 inches, and upon the minor axis 113 feet 6 inches; the curves being struck from two radii, one of 90 feet 3 inches, the other 50 feet 9 inches. The batter of the wall is 1 in 11. In structure the iron wall consists of 15 inches of iron in three thicknesses, in the following order:—a front armour of three tiers of 5-inch plates, 21 feet 9 inches long, and respectively 4 feet 9¾ inches, 4 feet 1½ inches, and 3 feet 5¾ inches wide (giving a total external height of 12 feet 5 inches); a backing of 6 inches of common concrete; next a layer of 5-inch bars 16¼ inches wide; these being crossed




MAP OF PLYMOUTH SOUND.

behind by another layer of similar bars laid horizontally; the whole supported internally by vertical iron bars, 5 inches thick and 12 inches deep, placed upright and edgewise and let into the floor through a footplate, as also into the roof at short intervals. The entire circumference is pierced, at distances of 21 feet 9 inches from centre to centre, by eighteen ports for 10-inch 400-pounder rifled guns, which when in position will point variously—some seaward, some across each of the two shipways, and others up the Sound; but in no case does it appear that a converging fire of more than four guns can be obtained.

This brief view of the situation will show that the passage of any hostile fleet would have to be made along the contiguous

waterways, and for deep-draught ships well within the thousand yards' range. In other words, the fort would have to stand its battering, if attacked under such circumstances, at uncomfortably close quarters. Strength and invulnerability ought, therefore, to be its primary qualifications.

In respect to strength, its very massiveness and weight of oval wall, must require, if even the materials were simply piled together, a very considerable force to throw them down; but materials cost money, and rolled iron in large masses a good deal, too, both for manufacture and carriage. Any proper system of iron-fort building should clearly, then, be based on the best mechanical plans of constructing with the use of the smallest amount of material employed in its cheapest forms. The structural cohesion of the whole fabric, and its power, in all its details, of clinging together, should also be leading features in all designs of this class, having in view the shattering and destructive effects of any heavy cannonade by modern artillery.

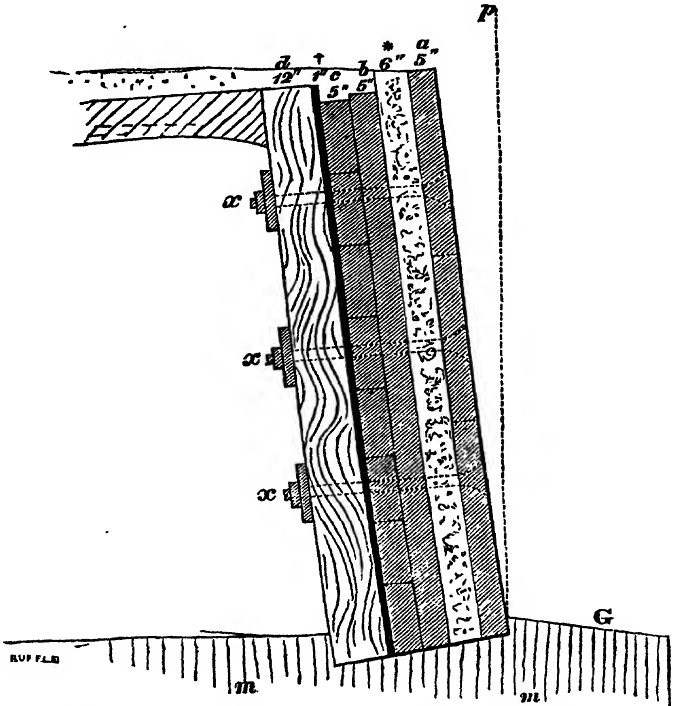
It is impossible, unfortunately, to take the new iron fort at Plymouth as an example of remarkable engineering, or of first-rate constructive or scientific skill; but it is, nevertheless, a topic of grave interest both in a national point of view and in respect to the immediate protection it is capable of giving to one of the most important naval arsenals of this country.

In the memorable contest carried on at Shoeburyness in 1868 an exciting interest was created between the rival targets constructed upon the plans of Colonel Inglis, of the Fortification Branch of the Royal Engineers, and that designed by Mr. John Hughes, with the hollow stringer backing; the work of the latter, victorious in respect to endurance and resistance, has already been described in this magazine (vol. vii. p. 345).

The design for the Plymouth iron fort, which at that time was embodied in one of the competing targets, has since been carried into execution upon the same main principles which at that date proved so deficient of strength and cohesion in the representative casemate. Various parts and details have been modified, as much as they could be, to bring in those improvements which the experience of the artillery attacks and the suggestions of criticism had dictated. We have, then, in the final production a formidable structure, although not a scientific example of the best order; but a fort, nevertheless, of considerable strength, and, for the duties it might at the present time be called upon to perform, sufficient probably in its essential qualifications. One of the main reasons constantly repeated for adhering to the defective principles of the fort was an asserted economy in the use of the narrow bars. If, however,

we put the cost of the Plymouth fort, as it now stands, with the estimate of a comparative structure upon Mr. Hughes's plan, we shall find that no saving has been effected by employing bars instead of hollow stringers; whilst the advantage of the latter would be very great in respect to defensive resistance.

The accompanying woodcut will show the general structure of the walls of the Plymouth fort as they now stand. The front armour (*a*) is of rolled plates 5 inches thick; behind this is a layer of shingle concrete 6 inches thick; next a layer of




SECTION OF IRON WALL, SHOWING SYSTEM OF STRUCTURE.

*a*. Face armour; * Concrete; *b*. Vertical bars; *c*. Horizontal bars; † Iron concrete; *d*. Wood plank, with iron struts at intervals; *x x x*. Bolts, backing; *p*. Perpendicular; G. Glacis; *m m*. Masonry foundation.

vertical iron bars (*b*) 16¼ inches wide and 5 inches thick; then a similar layer of bars (*c*) laid horizontally; behind this a plastering of iron turnings and asphalte 1 inch thick. In the rear the wall is supported by vertical iron bars 12 inches deep and 5 inches thick, the intervals (about 10 inches) being packed with wood planks set edgewise. A glance at the composition shows that there is not the slightest mechanical cohesion in it, but that the whole mass is simply kept together by the bolts. If these give way, the disruption of the wall seems inevitable.

Moreover, the bulging effect of projectiles striking the front will open out the incoherent backing, if it do not actually draw the bars and timbering out of their footings. The concrete behind the face-plating will be quickly disintegrated by any heavy battering, and then the bolts being loosened in their hold, the armour will clatter about in certainly a very noisy manner, if with no further damaging results; but how the multitude of bars will keep together in position under such circumstances must be left to the constructors to explain, or probably for Providence hereafter to determine.

In the Gibraltar shield, after the first failures of the bolts, others upon the pattern devised by Major Palliser were introduced. These had plus screw-threads, and were tapered away in the middle of the shank, so that when the strain was brought upon the bolt, the weakest part yielded and stretched. The objection to these bolts was, that they could not be used in ships, as there would be a cavity all round them. They stood, however, well enough in the land shield. In the Hughes shield there was another class of bolts of a much better character, founded by Mr. Parsons upon a similar basis. These were hollow cylinders with plus screw-threads, the cavities at the ends being closed by steel plugs. The cylinders or shanks, having thus the least substance stretched uniformly throughout, and being made of good ductile iron, the stretching limit was very considerable, and they were thus capable of withstanding admirably the strains brought upon them by the shot. They were cheap, too, as well as very strong. Subsequently, Lieutenant English, R.E., produced and patented a compound bolt with a carefully turned ball and socket-head on both ends, and a spiral spring-washer for buffing the rear nuts. The plan may be ingenious, but *cui bono?* If cheaper and less complicated bolts do the work required of them, why employ the costly which can do no more? None have ever stood severer tests than Mr. Parsons', or served their purpose better.

What the effect of a concentrated broadside of the heaviest guns might be on the new Plymouth fort would be very hard to estimate; and happily the heaviest guns at this moment are alone possessed by England. It will be remembered, however, that at the Shoeburyness trials a salvo of three guns broke an orifice in the 15-inch structure through which a General entered the casemate. No salvo was ever fired against the 20-inch portion there, and the doubt therefore remains whether a lamination of 15 inches of iron plus 6 inches of concrete will keep out a concentrated broadside from such ships as the *Hercules* or the *Devastation*. If so, the credit will be due to the improvements made in the mechanical distribution of the materials by the force of critical comments.

# SOUTH AFRICA AND ITS DIAMONDS.

By T. RUPERT JONES, F.G.S.,
PROFESSOR OF GEOLOGY AT THE STAFF AND ROYAL MILITARY COLLEGES, SANDHURST.

"PARKERTON is being surveyed, and before long it will have its Market-square, Church-square, High-street, West-end, back slums, and river villas. . . . The Church Committee are calling for tenders for bricks. . . . The Standard Bank premises will shortly be completed; and the British Commissioners' Offices will not be very much longer on the road." Store is added to store; and the jeweller's store "is as safe as if it were in the best watched site in London—nay, it is safer. . . At Pniel great attention is being given to sanitary measures. . . . Its natural situation entitles it to be called the very gate of the Diamond Fields. . . . Pniel and Parkerton are likely to live and flourish." "Hebron is to have its races on January 2," and Pniel also on the same day; whilst the necessaries and comforts of life, together with music-halls, liquors, quack-medicines, and other accompaniments of civilisation, are announced as ready to hand at the above-named settlements in the Klip-drift Diamond-fields, by the "Diamond News and Vaal Advertiser," No. 10, December 17, 1870, out of which paper and its "Supplement" (published at Pniel) the foregoing extracts have been taken. The newspaper and the nascent towns are, of course, due to the concourse of diamond-seekers, and of tradesmen supplying their wants, right and left of the Pniel Mission Station, near the "Great Bend" of the Vaal River, 480 miles north-east of Cape Town, upwards of 300 miles due north of Port Elizabeth (Algoa Bay), and about 290 miles west by north of D'Urban (Port Natal).

There are numerous spots at which local proprietors, squatters, and regular diggers have met with diamonds along the valley of the Lower Vaal, as at Hebron, Klip-drift (or ford), Pniel Mission-station and its neighbouring grounds, Bultfontein (or Du Toit's Pan), Zitzikamma, Vogelstruis Pan, Sitlacomie's

Village, Sifonell's Village, Gong-gong, Cawood's Hope, Nicholson's Pan, Kalk farm (Litkhatlong, near the junction of the Hart with the Vaal at the "Great Bend"), and elsewhere, near the junction of the Modder and Vaal. Also below the junction of the Ky Gariep (Vaal) with the Nu Gariep, to form the Gariep (Orange), diamonds have been found on several farms along the latter river, about twenty miles north-west of Hopetown. The finding-places, however, of the precious gem are not confined to the immediate neighbourhood of the Orange River and its great northern branch. The valleys of two, at least, of the tributaries of the latter (Vaal), namely the Modder and the Vet, have yielded specimens in their upper branches, near Fauresmith (eighty miles south of Pniel) and near Winburg (seventy miles from the Vaal) respectively. High up the Vaal, also, at Bloemhof, twelve miles south-west of Potscherfstroom, diamonds are said to occur; and even above Bloemhof some diamonds have been found (I am informed by Mr. C. L. Griesbach) on the Maquassi Spruit, a stream running into the Vaal from the north, and some 250 miles away on the north-east, from the Hopetown District. Throughout this region, however, no place has hitherto proved to be so rich in diamonds as the neighbourhood of Pniel above-mentioned. Hebron, an old mission-station, about ten miles up the river, on its north bank, and within a great loop of the stream, has been a productive digging-ground. The Klip-drift (or ford), by which the waggon-track from the south crosses into Betchuana Land, gives its name to the rich diggings five miles lower down the river than Pneil, mostly on the north side of the river, but including some grounds belonging to the missionary station, enclosed in a fine curve of the river, and traversed by the road to the south on its leaving the "drift." Four or five miles further down are the Gong-gong diggings, on the left bank of the river as it winds along on its north-westward course before receiving its northern tributary, the Hart, and making its "Great Bend" to the south-west; and the new diggings called Cawood's Hope are just opposite. All the diamond-diggings about Pneil, and to the north-east and north-west, are called the "Diamond-fields of Pneil," or "of the Lower Vaal," or the "Klip-drift Diamond-fields;" whilst the diggings near the ford indicate the "Klip-drift Diamond-field" in particular.

This rich diamond-bearing district, traversed by the winding Vaal, and suddenly occupied by an energetic digging and trading community from all parts of the world, belongs partly to the Pniel Missionary Establishment, within the limits of the Orange River Free States; but the north side of the valley* is claimed

* Referred to as "Adamanta" in the "Grahamstown Journal" of January 30.

both by native chiefs of the Batclapin (or Koranna) tribes, and by the Orange River Free States (lying south of the river), and even by the Transvaal Republic, whose main territory is higher up to the East on the north side of the Vaal. The rights of ownership and the demarcation of boundaries are to be settled by Commissioners or otherwise; and some kind of regular government has to be organised, in spite of the rowdyism of the gem-seeking cosmopolites and the disputes of the conterminous states and tribes, if the diamond-yielding character of the region be persistent, or if the labour and capital of the present settlers succeed, as is probable, in fixing civilised homes thus far in the Interior, in spite of the sandy and stony soil, and of the floods of one season of the year and the scorching heats of another.

Of the Klip-drift Diamond-field a surveyed plan has been sent to England by Mr. E. T. Cooper, one of the Government land-surveyors of the Cape Colony. It was published in the "Mining Journal" of March 4, 1871, and gives a good notion of the existing topography and drainage, and of their relation to the probable conditions under which the diamonds were deposited there.

At and near Klip-drift the river has an extremely winding course among somewhat flat-topped hills, a mile or so in their greater diameters, and varying from 300 to 450 feet in height, with gullies or creeks running down between them to the river. The tops and slopes of these hills ("Kopjes" they are locally termed) have been the chief sources of diamonds to the diggers. According to Mr. E. T. Cooper, writing in October 1870, Hond's Kopje, 400 feet high, has yielded possibly 15,000*l.* worth of diamonds; Rosa's Kopje, 400 feet, about 100,000*l.* worth (including a diamond of fifty-six carats); and Original Kopje, 300 feet, upwards of 100,000*l.* worth.* The yield of the hills on either side of the river at the drift or ford itself (on the mission ground 450 feet, and opposite about 50 feet high) is not specified.

The flats by the waterside do not appear to have been here successfully worked—only the sides and summits of the hills; and here the diamonds are found in a ferruginous gravelly alluvium,

* These estimated values for Klip-drift alone far surpass the declared value of the diamonds shipped to England, according to the statement in the "Times" for February 7, 1871. Klip-drift, rich as it is, has been deserted by the digger for Hebron and Cawood's Hope. The latter place is opposite Gong-gong, and the diggings are in recent alluvium, accumulated by the river between an island and the bank. This we learn, whilst this paper is in the press, from Mr. Barry's Lecture of January 27, reported in the "Grahamstown Journal," and reprinted in the "Colonial News" of March 17.

consisting mainly of lydite, jasper, agate, and quartz, with ochre and rotten felspar, mingled with large and small blocks of the felspathic amygdaloidal trap-rocks that constitute the bulk of each hill. Indeed this basement-rock is undergoing decomposition, some of it breaking up into rotten ochreous felspar, with frequent concretions of chalcedony (agate and carnelian), whilst other portions, less decomposable, remain in angular and exfoliating pieces.

At different spots, however, the gravel varies in its constitution. Rock-crystal is plentiful in some places, and amethyst also occurs. Garnet, peridot, mesotype, natrolite, and calcite abound here and there; but the exact conditions under which they are met with have not been noticed as yet. Brown mundic (hepatic pyrites), ilmenite, specular iron-ore, diopside, tourmaline, and diamond are rarer minerals in the gravel. Of nearly all the above-mentioned minerals there are water-worn, freshly broken, and perfect crystals. The chalcedony, also, and jaspers occur in both the worn and the unworn state.

It has been suggested by the late R. N. Rubidge and others that large areas of this part of South Africa have been at a long distant period (and yet *recently* as regards geological time) covered with alluvia, derived from the operations of water and weather on the vast region drained by the head-waters of the Orange and Vaal, and now represented in part by the great Quathlamba or Draakenberg. This mighty range and its southern spurs supply by far the majority of the sources of the present Orange River system, and still yield to the upper valleys agate gravel in abundance, from their amygdaloidal volcanic rocks. To the north, however, in the Transvaal, some of the head-waters of the Vaal (Ky Gariep) rise in the Gats Rand and other mountains, which consist of a different rock-system. To these allusion will again be made when we consider the probable origin of the diamonds. (See my paper on the Geology of the Diamond-fields of South Africa—"Geological Magazine," February 1871—for technical details and full references to published papers and other materials.)

The ancient alluvial plains, above alluded to, were probably terraced, according to our authors, by the successive subsidences of lake and river; and here and there they were at times coated with beds of calcareous tufa, derived from aggregations of fresh-water and land shells; and this still lies thick on many parts, and serves as a source of lime to the Colonists of the Interior. The work of natural denudation, or the remodelling of the surface by rain and rivers, progressed, whether aided or not by ice-action (as suggested of late by Mr. G. W. Stow for the surface-modification of the Stormberg and Queenstown country further south), and ultimately the levels of the present

drainage-system were arrived at; portions of the old high-level alluvium being left undisturbed on those stable portions of the valley-floor that resisted the wear and tear of denudation. If this be so—and it is likely enough—the precious gems brought to their places by the old system of drainage of the former high-level flats still lie amongst the remnants of local *débris* and ancient drift on the Kopjes, which remain like the navvy's "dead-men" of excavated plains, and in the fallen talus and running sand and gravel of their slopes, whilst few turn up at lower levels.

Nevertheless, there must be places further down the valley,* probably as "pockets," or patches of small extent, in or near the present river channel, into which the hard and heavy gems have been sorted (like gold-dust or tin-stone) from amongst the miscellaneous sand and gravel. Whether or no it will be worth while to cut off some bends in the river by short cuts, and work over the drained river-bed, energy and experience will probably some day prove.

But whence came the diamonds at first? And, if their origin can be traced, will it be profitable to look for them in their native matrix?

All the world knows that diamonds, whether in India, Borneo, Sumatra, South Australia, the Ural, Algiers, California, the United States, or Brazil, are got from alluvial gravel derived from more or less distant mountains. In Brazil only have these gems been found in their native beds, namely, in a granular quartzose schist (itacolumite), and some other schists (micaceous, chloritic, talcose, hornblendic, and argillaceous) associated therewith. These, together with some accompanying limestone bands, evidently represent, in metamorphosed (highly altered) conditions, some very old sandstones, clays, shell-beds, &c., such as constitute any one formation of marine and fluvio-marine deposits. The diamond crystals that occur in these Brazilian schists may also be, and indeed are always regarded as being, the results of some of the changes that have affected the strata in question; and they may represent the carbon of old carbonaceous deposits, separated and purified from hydrogen, clay, and other matters, whether within the original mass of the strata, or sublimed through pores and fissures from still more deeply seated sources of carbon.

It was suggested by the late Dr. Rubidge that the direct heat and pressure of volcanic dykes passing through coal-beds might bring about a change of hydrocarbons, producing pure carbon (=diamond), as they have changed certain coal-seams

* See the foregoing footnote.

in South Africa and elsewhere into coke, anthracite, and graphite, which are almost pure carbon. Although the chain of evidence is here incomplete, this hypothesis has ardent supporters in the Cape Colony, inasmuch as the trap-rocks above referred to as constituting a main portion, if not all, of the Kopjes at Klip-drift are, without doubt, of volcanic origin, whether they be dykes or outspread masses, and have passed through the fissured strata that here and there in the gullies are seen to lie beneath, forming the yet lower foundation of the district.

The low-lying strata, moreover, are known to be a part of the great stratified formation that crops out along the hill-sides south of the Orange River basin, in the Colesberg, Smithfield, Harrismith, and other districts to the south and east, and which, indeed, constitutes all the Interior of the Colony within the circling ranges of Namaqualand, the Bokkeveld, Zwartebergen, Winterhoek, and Zuurbergen, ending near the mouth of the Great Fish River in Albany, on the south-eastern coast. Within this great area the nearly horizontal and probably lacustrine (Triassic) strata, first examined, defined, mapped, and described by the late A. G. Bain, spread far and wide; and their geological name, "Karoo Formation," is derived from the Great Karoo Desert, which is a characteristic feature of a considerable tract in the Worcester and Beaufort Divisions. They are also known as the '*Dicynodon* strata,' on account of the prevalence of the remains of that remarkable two-toothed reptile in some of the beds. The Karoo Formation consists of an enormous series of shales and sandstones, accompanied by some calcareous bands, and rich at places with the wonderful remains of the above-mentioned reptile and many others; also with fishes of the palæoniscan type, together with seams of lignite and coal, remains of coniferous trees, ferns, and other plants. Throughout their whole extent these Karoo strata are crossed by frequent dykes of doleritic, dioritic, and syenitic trap-rocks, at different angles, and are often overlain by, or intercalated with, similar igneous rock. Here, then, are some of the elements required in Dr. Rubidge's hypothesis of the formation of diamond from coal by volcanic interference; but direct proofs are altogether wanting. We see here also the reason why many of the Colonists, who have read geological books, and observed something of the structure of their country, are so ready to suppose that the diamonds are native to the spots where they are found—converted, they imagine, out of hidden coal-beds and plant-remains by the subterranean heat, of which, truly enough, the volcanic rocks bear witness.

Let us look again at the accompaniments of the diamond crystals at Klip-drift and thereabouts; and although the agate

and carnelian, the peridot, mesotype, natrolite, calcite, and probably the rock-crystal and amethyst, have been derived from the veins, glodes, and kernels of the amygdaloidal and other trappean rocks *in place*—and we may add the garnet too, for I have a specimen of melaphyre (?) loaded with garnets, labelled "from near the Orange River"—yet the lydite must have come from some old metamorphic rocks; and we may associate with it the hepatic mundic, the ilmenite, specular iron-ore, diopside, tourmaline, and most probably the *diamond* as well.

Many of these specimens, not being waterworn, could not have travelled far. There are, however, two very probable local sources for them: namely, 1st, Outcrops of old rocks, still lower in the series than the Karoo beds which lie on the floor of the valley, pierced and covered by the greenstone and amygdaloidal lavas; and such outcroppings are indicated here and there in the Orange River Free States and the country to the west. 2nd, The blocks and smaller fragments of old rocks, constituting the materials of some of the Karoo beds themselves.

We must further observe that, on the one hand, the great Draakenberg, based, no doubt, on old metamorphic rocks, such as Sutherland and Griesbach have met with on the Natal side of that range,* has supplied, and may still supply, to some of its rivers the minerals of the older series of rocks, as well as of the Karoo beds and their dykes. On the other hand, the northern head-waters of the Vaal come direct from off old metamorphic rocks, *and have some diamonds in their valleys before they reach that part of the Vaal which flows over Karoo strata.*

We may add that down all the valleys of the Orange River-system ice may have played its part, as among the hills further south, and have carried blocks and crystals for miles, and left them to be detached, unharmed, by subsequent operations of rain and rivers.

Lastly, what are the metamorphic rocks of the Transvaal and the Upper Vaal, on the one hand, and of the base of the Draakenberg, on the other? We know that "Devonian" schists, supporting an old sandstone, are present in the latter, and that similar rocks lie against still older mica-schists, marble, gneiss, and granite in the Gats Rand about the head-waters of the Vaal. And, further, we now know that the Karoo beds of the Stormberg, between Washbank and Queenstown, lie on the palæozoic carboniferous strata containing *Lepidodendron*, *Sigil-*

* With similar rocks on this eastern side of the range, probably diamonds may also be found in the alluvium of the valleys.

*laria*, *Calamites*, and coal; and in Natal they lie unconformably against this old carboniferous formation, igneous rock intervening. If, then, these palæozoic coal-beds, stretching further to the north-east, have participated in the crumplings, squeezings, and slow changes of the crush and metamorphism affecting the deep-seated Devonian and other rocks, they are likely enough to have yielded their carbon to the mica-schists, clay-slates, gneiss, and quartzites, not only as graphite, but as *diamond*; whilst the other elements of the altered strata went to form the mundic, ilmenite, tourmaline, and other minerals known to have been produced by similar metamorphism in the diamantiferous rocks of Brazil.

It is not found to be profitable, however, there to work these gem-bearing rocks, though softened by atmospheric and aqueous agencies. The grand machinery of the rivers has carried down and sorted the material better than man can do it. We should not, therefore, be certain, if the birthplace of the South-African diamond were found either in the Gats Rand, at the roots of the Draakenberg, in some isolated patches of old rock in the Orange River basin, or in the material of the Karoo beds themselves, that it would be at all profitable to attack it with pick and shovel, or with other machinery. The more scientific plan would be for a clear notion of the stratigraphical structure of the whole country, and of its geological history, to be worked out by a thoroughly educated observer; whereby the place of origin, the mode of transport, the sites of deposit, and probably of re-deposit, of the coveted gems should be satisfactorily mastered. This would be a pleasing task, and a long work, for a hard-muscled, fever-free, enthusiastic young geologist, willing to do the Colony, nay, the world, a service, with little hope of monetary reward. Without waiting, however, for this perfect and disinterested guide, and for many a year to come, the bold empiricism and blundering pluck of the white man will snatch the bigger gems and bury myriads of good diamond grains, and, may be, other jewels, in acres of heaped rubbish, for John Chinaman some day to sift. And fortunes will have come to few and misery to many, but average means of existence to the mass of those who venture on the doubtful grounds of hidden wealth, digging and digging until the soil, as usual, gives its return of crops and herds, instead of the hoped-for, but less desirable, gold or gem.

# REVIEWS.

## THE BONES OF MAMMALS.*

MR. FLOWER took the subject of the mammalian skeleton for his first course of lectures at the College of Surgeons. The subject was hardly, one might think, a novel one; yet the author showed, by the number of his researches and the peculiar views which he expressed, that it was one in which, as in every other department of comparative anatomy, originality might be shown. But, apart from this view of the matter, we think that Professor Flower has done well; for he has given the student—what he could not obtain before in a similarly easy manner—a sketch not only of the human skeleton, but also of the various forms of the mammalian type, which differ more or less from the human arrangement. Unlike many, who would have thought the skeleton of man unimportant, he places it first under each section, and then describes the typical forms he has chosen to represent the orders of mammalia. Some may imagine that a book merely on the bones of mammals must be necessarily dry and uninteresting. It is not so, however; and when one remembers that in all our fossils we have nothing left but a congeries of bones, he sees the immense importance and profit derivable from such a pursuit. It is, in the opinion of many comparative anatomists, of no consequence at what part of the scale we begin, whether among the *monotremata* or man. But we think Mr. Flower is just in his conceptions on this point. He says "the structure of man has undoubtedly a more universal interest than that of any other organised being, and has therefore been more thoroughly worked out; and, as the majority of terms used in describing the parts composing the bodies of vertebrate animals were originally bestowed on account of their form, relation, or real or fancied resemblance to some object, as they were met with in man, there are advantages in commencing with members of the highest class, and mastering their essential characters before proceeding to acquire knowledge of the other groups." In this we quite agree, and although there are simpler forms, yet man's osteology is so familiar to every student that

* "An Introduction to the Osteology of the Mammalia; being the substance of the course of Lectures delivered at the Royal College of Surgeons in 1870." By William Henry Flower, F.R.S., F.R.C.S. London: Macmillan, 1870.

it naturally comes first. The author, in classifying the mammals, follows De Blainville's system, and divides them into *Monodelphia*, *Didelphia*, and *Ornithodelphia*, the two latter representing respectively the *marsupialia* and *monotremata*, and the first including the remaining mammals. Of the principal mammalia he takes the following views. He places the *Edentata* quite by themselves, as a low order of the mammalian division. The *Primates* include man and all monkeys, and with some doubt he places also among this group the *Lemurina*. Next the *Chiroptera*, or bats. Then the *Insectivora*, or hedgehogs, shrews, moles, &c. The *Carnivora* he divides into *Fissipedia*, including the cats, dogs, bears, and their various modifications, and the *Pinnipedia*, including the seals, walrus, and eared seals, or sea-lions, as they are termed. The *Cetacea* contains two sub-orders—the *Mystaceti*, or whalebone whales, and the *Odontoceti*, including the cachalots, narwhals, dolphins, and porpoises. Then come the *Sirenia*, including the manati and dugong. Next the *Ungulata*. These he divides into the *Perissodactyla*, or odd-toed, containing the horse, tapir, and rhinoceros, and the *Artiodactyla*, which he subdivides into four sections—(*a*) the non-ruminating, including the pigs, peccaries, and hippopotamus; (*b*) the cushion-footed, or *Tylopoda*, the camels, and llamas; (*c*) the *Tragulina*, a group of little deer-like animals, formerly placed with the musk-deer; (*d*) the *Pecora*, including the deer, giraffes, antelopes, sheep, goats, and oxen. Next comes the order *Hyracoidea*, including alone the genus *Hyrax*. Then the *Proboscidea*, including the Asian and African elephants; and, lastly, the tenth order, the *Rodentia*, embracing the hares, rats, guinea-pigs, porcupines, beavers, squirrels, &c. The animals in the two other divisions we have already stated. On the contents of the volume it is as unnecessary as it would be out of place for us to enter. It is admirably lucid and wonderfully condensed. The section on the skull is a marvellous chapter; it contains so much in so little space. The illustrations are remarkably clear, and have most of them been drawn for the present work; they are 126 in number, and they very fairly represent the osteology of the whole mammalian group. We hope the book may have a large sale, for it is literally the only thing of its kind in our language.

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## NATURAL PHILOSOPHY.*

WE have had more than enough of treatises on Natural Philosophy. Yet we have had very few to which we have been able to award even the most distant praise. Here is another treatise adapted, it is said, for the use of schools and junior students, and what shall we say of it? In the first place, we must observe that it is adapted to the use of schools alone. As a book for students—such for instance as first year's university men—it is quite unsuited to the end; it is a thousand times too simple and elementary. For schools, however, we think it a very good book,

* Elementary Natural Philosophy; being a course of nine Lectures specially adapted for the use of Schools and Junior Students." By J. Clifton Ward, F.G.S. London: Trübner, 1871.

better than most of its kind, and it gives us great pleasure to express our approval of it. It does not possess that quality which so many works of its class exhibit, viz. the manufacture from three or four standard treatises. Of course Tyndall's and such-like books have been referred to and taken from largely. Yet we think the author has selected well from the sources to which he has gone, and we think he is entitled to praise for his efforts. His style, too, is short and clear, and he leaves no stone unturned for his pupil, but endeavours to his utmost to make the subject intelligible. The woodcuts are very simple, but they are sufficiently numerous. The author has dealt with electricity, magnetism, light, sound, heat, and pneumatics and hydrostatics. He has discharged his task simply and well. There is only one thing to be regretted, and that is a list of instruments, &c. which is furnished by Messrs. Horne and Thornthwaite. This is ridiculously expensive, and is altogether out of keeping with such a work, seeing that it reaches to no less than 82*l.*

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## STRANGE DWELLINGS.*

OF the many writers of popular natural history works which there are abroad, and the multitude of works they each of them publish, there are not many that possess excellence or accuracy. But when we look at home what do we see? Can we assert that the writers who produce those handsome popular works are a whit more accurate in England than in France or Germany? We fear we cannot do so to any great extent. Whether it is Mr. Wood or M. Figuier, we fancy there is not much to choose, though we imagine that our English author of natural history books cares more for truth than does the Frenchman. At all events, there is no doubt at all that Mr. Wood is a marvellous book-maker, and there is little question that though some of his books may contain errors, yet that these are few, and are partly compensated for by the admirable manner in which he describes his facts and figures his specimens. The book before us contains some 400 pages, and is practically an abridgment of his very best work—"Homes without Hands." We ourselves do not see the necessity of an abridgment. "Homes without Hands" was an admirable volume, exceedingly well illustrated, and, so far as we could see, by no means too large. But of course the author has had his own experience, and we cannot blame him for producing a smaller and cheaper edition. The work contains descriptions of animals only in reference to their homes, and these are clearly and fully described and figured. The sketches appear to us to be abbreviations of the larger ones in the parent volume, but they are not good cuts; indeed, this seems to us to be a point on which the author should be blamed, for the illustrations, as compared with those of some of the French volumes reproduced in this country, are far inferior—they are thin and bald, if we may use the terms, and in many

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* "Strange Dwellings; being a description of the Habitations of Animals." Abridged from "Homes without Hands." By the Rev. J. G. Wood, M.A., F.L.S. London: Longmans, 1871.

cases are very badly printed. The book classifies animals according as they (1) burrow in the ground; or (2) suspend their homes in the air; or (3) build them of sticks, mud, stone, and so forth; or (4) make their habitations under the water; or (5) live socially in communities; or (6) are parasites upon other animals or plants; or, lastly (7), those which build on branches. His account of the habits of some animals is very interesting. Thus, speaking of the curious robber-crab of the Indian Ocean, a creature whose gills are kept moistened by a store of water, which enables it to live for a whole day of twenty-four hours without a visit to the ocean, he quotes from Mr. Darwin, and tells us that the species "seizes upon the fallen cocoa-nuts, and with its enormous pincers tears away the outer covering, reducing it to a mass of ravelled threads. This substance is carried by the crabs into their holes for the purpose of forming a bed, whereon they can rest when they change their shells: and the Malays are in the habit of robbing the burrows of these stored fibres, which are ready picked for them, and which they use as junk. . . . When the crab has freed the nut from the husk, it introduces the small end of the claw into one of the little holes which are found at one end of the cocoa-nut, and, by turning the claw backwards and forwards, as if it were a bradawl, the crab contrives to scoop out the soft substance of the nut." If we mistake not, an exquisite woodcut accompanied this account in the parent volume, which we wish the author had introduced into this. As we have already said, the book is a good one, but the illustrations are bad; and that, in these days of elaborate woodcuts, is a fault for which we think Mr. Wood cannot be blamed too much. Description is very good in its way, but illustration fixes it on the mind for ever.

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## THE STUDENT'S GEOLOGY.*

THIS, which appears a new work, and which in point of the novelties of geology with which it abounds, is really so, is nevertheless but a new edition of a work which every biologist is already familiar with—the "Elements" of Geology. The "Elements" was, as the author states, out of print in 1868, and he set to work with the idea of bringing out a new edition. But various influences were at work upon him to induce him "not to repeat those theoretical discussions, but to confine himself, in the new treatise, to those parts of the 'Elements' which were most indispensable to a beginner." This was, in fact, to revert to the first edition of his work. It was a difficult task: many chapters had of course to be recast, figures of particular specimens had to be cancelled and replaced by others, and additional illustrations had to be brought in. Nevertheless, Sir Charles Lyell set to the task, and in bringing out the present new series—"The Student's Elements of Geology"—he has discharged a task for which he must receive the grateful thanks of all who study geology, and of all those who wish to see the

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* "The Student's Elements of Geology," by Sir Charles Lyell, Bart., F.R.S. London: John Murray, 1871.

science advanced. We think, too, that the wants of the reader are far better supplied than the modest author imagines when he refers, in illustration, to the "old woman in New England who asked a bookseller to supply her with the cheapest bible in the largest possible print." We look upon this last book of Sir Charles Lyell's as a perfect model, for it contains almost everything that the geologist who is not engaged in advancing the study of geology should know, while for the student it contains everything which he requires. We admire, too, the way in which the author has been at pains to illustrate everything, conscious, doubtless, from long experience, of the great necessity of having multitudes of wood-engravings in a text which will be chiefly read by those commencing the study of this most entrancing and wonderful science. This will be the more evident when we say that the text extends over 600 pages, and the woodcuts reach no less a number than 630. It would, of course, be impossible to review a book like this beyond merely stating our opinion of it, as we have done. But, however, we may refer to one or two sections, showing how the author has brought in the most recently ascertained facts within the compass of his space. Especially is this shown in his references to Carpenter and Wyville Thomson's idea of the present existence of the chalk formation, an opinion founded on the presence in the Atlantic of a few chalk shells hitherto considered fossil. "We must be careful," says Sir Charles Lyell, "not to overrate the points of resemblance which the deep-sea investigations have placed in a strong light. They have been supposed by some naturalists to warrant a conclusion expressed in these words: 'We are still living in the Cretaceous Epoch'—a doctrine which has led to much popular delusion as to the bearing of the new facts on geological reasoning and classification. The reader should be reminded that we have been in the habit of founding our great chronological divisions not on foraminifera and sponges, nor even on Echinoderms and corals, but on the remains of the most highly organised beings available to us, such as the mollusca, these being met with in stratified rocks of almost every age. In dealing with the mollusca, it is those of the highest or most specialised organisation which afford us the best characters in proportion as their vertical range is the most limited. Thus the Cephalopoda are the most valuable as having a more restricted range in time than the Gasteropoda, and these again are more characteristic of the particular stratigraphical subdivisions than are the Lamellibranchiate bivalves, while these again are more serviceable in classification than the Brachiopoda, a still lower class of shell-fish, which are the most enduring of all. When told that the new dredgings prove that 'we are still living in the Chalk Period' we naturally ask whether some cuttle-fish has been found with a belemnite forming part of its internal framework? or have Ammonites, Baculites, Hamites, Turrilites, with four or five other Cephalopodous genera characteristic of the chalk, and unknown as tertiary, been met with in the abysses of the ocean? Or, in the absence of these long extinct forms, has a single spiral univalve or species of cretaceous Gasteropod been found living? Or, to descend still lower in the scale, has some characteristic cretaceous of Lamellibranchiate bivalve . . . been proved to have survived down to our time? . . . It has been very generally admitted by conchologists that out of a hundred species of this (Brachiopod) tribe occurring fossil in the Upper Chalk, one, and one only,

*Terebratulina striata*, is still living, being thought to be identical with *T. caput serpentis*. Although this identity is still questioned by some naturalists of authority, it would certainly not surprise us if another lamp-shell of equal antiquity should be met with in the deep sea." We have given at length Sir Charles's words because they have so much weight; not simply because they are his, but because of the weighty evidence they bring forward. We do not see how they can be received with the slightest doubt; and we are the more surprised that Dr. Carpenter, although no geologist, should have countenanced such a view, the more so as there is no reason to believe—indeed much against the belief—that the chalk has been continuous down to the present. In fact, to use Sir Charles's concluding remarks, "To talk of the chalk having been uninterruptedly forming in the Atlantic from the Cretaceous Period to our own *is as inadmissible in a geographical as a geological sense.*" We have been unable, from want of space, to say a word of the many valuable discoveries of Dr. Falconer in India, of Professor Heer's observations on the Upper Miocene of Switzerland, or of Mr. Carruthers' numerously quoted observations on fossil plants. But we mention them as particularly worthy of note, and thus we close our notice of one of the finest text-books which for years has appeared in this country.

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## THE SUN.*

THE author of this work is, we say it without the slightest ill-feeling, or the remotest tinge of satire, a most remarkable man. We say this, because within the last two years we have witnessed some of his work, and because we are taken with the most intense surprise at the vastness of his labours. In our last number we noticed briefly an extensive work entitled "Other Worlds than Ours," and now we have before us a still larger one of nearly 500 pages, on the most interesting, as it is certainly now the best examined, subject in Astronomy—the Sun. And it must not for one moment be imagined that this is a mere popular work. It is indeed rather more technical, although it is addressed to the general public. A work which deals with the subject of spectroscopy in the most recent manner, with the theories of the sun's surface, taking into ample discussion the views of Loewy, De la Rue, Secchi, Nasmyth, Dawes, and Stewart, which goes into the terribly difficult problems of prominences and chromosphere, dealing with the observations of Young, Dr. Zöllner, and Respighi, and, lastly, which narrates the several results obtained as to the corona and zodiacal light, is not a book which can be hastily read, or one for which the reader is not highly indebted to the author. Now, such a work is that which Mr. Proctor has set before us. We can of course notice but a very feeble portion of so deeply interesting a volume, though we wish we had space to go into a thorough review of it. But we may notice from the book the author's account of Professor Respighi's researches as to those singular projections of luminous matter from the sun which have of late years attracted so much attention, and created so much discussion among astronomers. Professor Respighi thinks

* "The Sun: Ruler, Fire, Light and Life of the Planetary System." By Richard A. Proctor, B.A., F.R.A.S. London: Longmans, 1871.

the prominences have no analogy to clouds, but are mostly phenomena of eruption. Where there are faculæ, there are also prominences, but they are not identical with each other. Over sun-spots there are low jets, but no high prominences. In the circumpolar regions the prominences are few, small, and last a short time. At the solar equator they are less active and frequent than in higher solar latitudes. He noticed some prominences exceeding three minutes, or ten terrestrial diameters in altitude, and one prominence had an elevation of no less than 160,000 miles. Professor Respighi found that the formation of a prominence is usually preceded by the appearance of a rectilinear jet, either vertical or oblique, and very bright and well defined. It rises to a great height, then bends back again, and ends by falling into the sun, sometimes forming a cloud in doing so. It is in the upper part of such prominences that the most remarkable and rapid transformations are seen, but a great difference is observed in the rate with which prominences change figure. Their duration, too, is variable—some are minutes, some even days in existence. The Professor thinks, says Mr. Proctor, that "the sharply-defined bases of the eruptive jets prove that the eruption takes place through some compact substance forming a species of solar crust. He agrees with Zöllner in considering that the enormous velocity with which these gaseous matters rush through the solar atmosphere implies that the latter is of exceeding tenuity."

It is out of our power to go further into the substance of this book. The quotation we have given shows the character of the volume, and we can only say that it abounds in novelties to the general reader as striking as the foregoing. It is elaborately illustrated, the coloured lithographs of the spectroscopic observations being especially good, and being valuable to the general student because of their absence from all works with which he comes in contact. In point of style the work is everything that could be desired. In conclusion, we must say a word of praise for the publishers and printers—the book is admirably got up, both as to paper, print, and illustration.

## THE HONEY-BEE.*

THE first edition of this work was published, we believe, in 1838, and was dedicated to Her Majesty. The present edition has been carried out by a dear friend of the author, whom he had arranged with to complete the work after his death. Yet we confess our inability to see any great advances which have been made. We do not mean that the present work is not a larger and more important volume than its parent; but what seems to us is this—that, so far as recent work is concerned, it really is most defective. There is an endless amount of quotation from Aristotle and Huber and the several old anatomists who did so much good work. But we do not find any account of recent researches, at least anything worth speaking of. The editor says he has been abroad, and it surprises us that he has not heard of

* "The Honey-bee: Its Natural History, Physiology, and Management." By Edward Bevan, M.D. Revised, enlarged, and illustrated by William Augustus Munn, F.R.H.S. London: Van Voorst, 1870.

the multitudinous work and experiments of the German bee-masters. Many of the statements which the volume contains have been long ago shown to be incorrect. In no case is this more marked than in the anatomy of the bee; the author's most recent authority seeming to be Cuvier. Indeed, it would seem to us as though the editor's "bar and frame hive" was the only novelty in the volume, and was the sole cause why a new edition has been brought out. No doubt the opinions of the older anatomists and bee-cultivators are of interest, but that they are now completely out of date there is not the least doubt in the world. For instance, it is perfectly absurd to make up a book by inserting the matter which has filled various works during the last century or so. If we may so express ourselves, the book is too much of the type of "Kirby and Spence," and has the disadvantage of being vastly older. We certainly do not regard it as in any shape or form worthy to be styled a modern work upon the subject it is supposed to treat. Doubtless, it contains an immense store of facts; but then these are to be found in every work of the kind published for the last fifty years, and, what is worse, a very large number of them are utterly unreliable.

## POPULAR PHYSIOLOGY.*

IT is surprising, but not the less true, that, great as one would expect the interest to be produced by a knowledge of the human frame, and interesting as one would imagine the study of the human frame to be, nevertheless books on physiology have almost always been a failure so far as the general public is concerned. Great as may be their wonder when some simple phenomenon in the human frame is explained, and intense as may be their interest under peculiar circumstances in knowing even the most elementary part of the human structure, yet, nevertheless, when good books, amply illustrated, are placed within their reach by popular writers, they are seldom or never purchased, and they almost invariably cause a dead loss to either the author or the publisher. Holding these views, we cannot hope for a very extensive sale of the work before us, and this opinion becomes the stronger when we remember the exceedingly small number—forty-five in all—of illustrations which it contains. Of course we must not expect very much accuracy in the illustrations to a work which is popular and from the French. The woodcuts are fair, but they convey a very inaccurate idea of the structure for which they are intended, and we fancy that M. Leveillé, the artist, did not engrave them in many cases from actual specimens, but from drawings; in fact, the sketch of bone-structure which is on one of the pages, is a purely imaginary combination of parts, and indeed the same may be said of the "rods of Jacob" in the section on the eye. Still, on the whole, the descriptions are very good, and have been rendered into admirable English by the translator; so that altogether the book is an excellent one, especially for the artist class. We wish it success.

* "Wonders of the Human Body. From the French of A. Le Pileur, Doctor of Medicine." London: Blackie and Son, 1871.

## THE ANIMAL KINGDOM.*

UNTIL Professor Owen's great work came out in its three volumes, Rymer Jones's work was the best which we had in our language upon the entire subject of comparative anatomy. Even still it is the most comprehensive book we possess on the subject of the Invertebrate sub-kingdoms of the animal world. Being written in a style which very few of our science men possess—a style which is simple and terse, yet wonderfully descriptive and telling—it possesses for the general student many advantages, for the writer makes matters as clear and intelligible as they can possibly be; it possesses, moreover, nearly six hundred admirable engravings taken from the best writers of the time. It is therefore an excellent work so far. But when we ask how much has it been brought up to the present day, we are astonished at the answer we are obliged to give. In no respect does it represent zoology as it now is. In hardly a single instance has the author taken even the faintest trouble to correct or modify his remarks, and though he tells us that he has done this in regard to the *Protozoa* and *Cœlenterata*, we can see not the least evidence of his having gone into the matter as he should have done. The same figures, the same letters, which twelve or thirteen years ago conveyed the most erroneous ideas, convey them still without the faintest alteration or emendation. This is really too bad in a work professing to be a new edition, and published in 1871. The book is simply *tout entier* the same work which was published we fear to say how many years ago. If Professor Jones wished to bring out a proper new edition of his work, we doubt not he could have found abundance of young, zealous, and accomplished men to have undertaken the task. As it is, he has brought out an old work in a new cover, and endeavours to lead us to believe that it represents the history of the animal world as we know it in 1871. This has been a sad mistake, and one we fancy by which the publisher must most seriously suffer.

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## THE PROGRESS OF MEDICINE.†

EVIDENTLY the author of this work tries to do his best. He labours very extensively to bring out a Report which shall be useful. Yet he fails most seriously. His work is a nondescript collection of all kinds of matters relating to medicine, and no one part is complete. The half-yearly volumes which we have been accustomed to so long are infinitely and

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* "General Outline of the Organisation of the Animal Kingdom, and Manual of Comparative Anatomy." By Thomas Rymer Jones, F.R.S., Professor of Comparative Anatomy in King's College, London; late Fullerian Professor of Physiology to the Royal Institution of Great Britain. Fourth edition. London: Van Voorst, 1871.

† Dr. Dobell's "Reports on the Progress of Practical and Scientific Medicine in different parts of the World." Contributed by numerous and distinguished coadjutors. Vol. II. (from June 1869 to June 1870). London: Longmans, 1871.

immensely before this volume. Like one who does not understand the nature of his task, he is led away by any great name. For instance, in the present volume, Professor Villemin is allowed to take up nearly one-sixth of the space with an account of French anatomy and physiology, while there is hardly a line on German work, which is about six times as vast, and much more of which ought to be in this treatise. When we tell our readers that Dr. Dobell relates the progress which this country has made during a whole year in anatomy and physiology, normal and morbid, in chemistry, and the whole of the practice of medicine and surgery in about 200 pages, we have said enough to show the intense vanity and the extreme ignorance exhibited by such a scheme. The book has really very little value, save and except to the author, who, notwithstanding the advice we offered when the first volume appeared, seems still desirous of showing the world another one. Doubtless many medical men think it a really good book; but those who are familiar with the real amount of work in the several departments, both at home and abroad, must regard it as the most imperfect, incomplete, and vain record of the labours of the profession.

## SCIENCE GOSSIP.*

HARDWICKE'S "Science Gossip" has completed its sixth volume, and we must compliment its editor, Mr. M. C. Cooke, M.A., upon the result. Mr. Cooke has done good work, and the public appreciate his labours, as we are very glad to proclaim. Indeed, in this volume it is hard to see that anything is neglected which could interest the reader. Every conceivable subject in general natural history is dealt with, and that too well and fully, while at the same time those who care for "gossiping natural history" will find it abundantly in the "Notes and Queries," which abound in interesting jottings about animals and plants. Of the many valuable and interesting papers, there are one or two to which we would call attention. One of them is called "The Towing Net," and is from the pen of Major Holland. It contains a miscellaneous store of knowledge, conveyed in a style most praiseworthy. We do not say that it is excessively scientific, but it is, we think, just the species of paper for "Science Gossip." Another very excellent paper is one entitled "Eggs of Butterflies and Moths." Unhappily the author's name is not given; but we cannot pass it by without noticing that it contains a mass of information, conveyed just as such knowledge should be conveyed. We have looked in vain for faults; all through the journal strikes us as a perfect one, and one which distances competition by the marvellous price at which it is published.

* Hardwicke's "Science Gossip." An Illustrated Medium of Interchange and Gossip for Students and Lovers of Nature. Edited by M. C. Cooke, M.A. London: Hardwicke, 1871.

## ESSAYS ON DARWINISM.*

MR. STEBBING has done, we think, wisely in reprinting his series of essays, and although some of them only relate to Darwinism indirectly, yet are they very fair arguments in favour of the philosophy of the great master; and being moreover couched in homely language, and being short, they are likely to prove an introduction to Darwinism in many cases. We find it impossible to go into the several chapters of which the work consists, but it certainly strikes us that the first, which relates to some of the difficulties which reasonable people have in accepting Darwinism, is particularly good and clear. The author, of course, does not touch any of the deeper arguments which underlie Mr. Darwin's theory, and which are yet really undecided, which even Mr. Darwin himself cannot get over, and which stand in the way of the general acceptance of his views. But he glances at the stronger arguments in favour of the doctrine, and he argues clearly and truly in most winning style. His book may be strongly commended to those who have not yet taken up any of Darwin's works, or who fail to understand them.

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## SCHOOLS FOR THE PEOPLE.†

WHILE now there is so much talk upon this subject—while Professor Huxley, leader of the scientific world, is contesting with those numbers of men who delight in doing nothing save what their ancestors have done before them, and who abhor that which is new—the book before us is one to be read by every person desirous of information on so vast a subject. It is a large, well-illustrated by page-plates of several schools, and handsomely got up volume. It deals succinctly and briefly with the several schools which it describes, and it is arranged so that the reader may if he choose go hurriedly through it, and pick out the black-letter headings of any department he may select. It treats of elementary schools in connection with the Committee of Council on Education, of schools in connection with the Science and Art Department of the Council, of schools under the Admiralty, of those under the War Secretary, of those under the direction of the Home Department Secretary, those under the Poor-Law Board, those under the Commissioners of Lunacy, of schools not aided by public grants, and lastly, of training colleges of the Committee of Council. Under these several heads there is given a great deal of information, which will be invaluable to members of the new School Boards, and all who have to deal with education under the existing changes. Mr. Bartley deserves the best thanks of the people for undertaking so vast a labour, and he has

* "Essays on Darwinism." By Thomas R. Stebbing, M.A., late Fellow and Tutor of Worcester College, Oxford. London: Longmans, 1871.

† "The Schools for the People; containing the History, Development, and Present Working of each description of English School for the Industrial and Poorer Classes." By George C. T. Bartley, Examiner Science and Art Department. London: Bell & Daldy, 1871.

our extreme gratification at the manner in which he has discharged an extremely difficult task. His work must be read by all engaged, either directly or indirectly, with education.

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## THE GENESIS OF SPECIES.*

AMONG the numerous opponents of Mr. Darwin's doctrines—and that they have been numerous a search through our literature for the past few years will demonstrate clearly—there were none worthy of consideration till Mr. Mivart appeared upon the field. And he has come with so much force, so much knowledge from reading, so much practical acquaintance with the subject of natural history, that anything which he may have to say will have to be heard with gravity and thoughtfulness. But apart from these, the author's characteristics, the volume itself is one which, from the closeness of its argument, the multitude of examples it contains illustrating its author's opinions, the great incompleteness of the Darwinian theory which it conscientiously exposes, and, above all, the calm, dignified, and conscientious tone which prevails throughout its pages, has qualities that in the highest degree commend it to the general and scientific reader. For ourselves, we may as well say at the outset, that it has not convinced us of the force of the views which it endeavours to prove, but we must equally admit that we have learned much from it that our mind had failed to take in before, and that it has led us to admit a view of natural selection more extended, vastly, than that to which Mr. Darwin's works give place. It appears to us that in admitting this extension of "natural selection," we admit all that is necessary to prove in favour of Mr. Darwin's dictum; but we confess that it is alone to Mr. Mivart's efforts that we have altered our opinion so seriously on this one point. In reference to the latter portion of Mr. Mivart's book and his opposition to Pangenesis, we differ entirely from him. With the theological part of the volume we totally disagree, but that may be disregarded in the present place; but we think his arguments against Pangenesis of the very feeblest description, and we could have wished them excluded from so admirable a volume. The one fact that a particle which can be merely seen by our eyes, may be practically as large as the whole world, and is infinite in division, is of itself sufficient in support of the principle which Mr. Darwin holds, while in almost every other point the doctrine of Pangenesis serves to explain phenomena, and without it, it appears to us, we are without any reasonable explanation of a whole host of natural facts.

We cannot, of course, in our short space do anything like justice to so admirable a book, for we can in no case give a statement of the author's many and closely-reasoned arguments. But we may briefly state what he attempts to prove in the course of the closely-reasoned pages of which his work consists. The following is a brief summary of his case: "That

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* "On the Genesis of Species." By St. George Mivart, F.R.S. London: Macmillan, 1871.

natural selection is incompetent to account for the incipient stages of useful structures. That it does not harmonise with the co-existence of closely similar structures of diverse origin. *That there are grounds for thinking that specific differences may be developed suddenly instead of gradually.* That the opinion that species have definite though very different limits to their variability is still tenable. That certain fossil transitional forms are absent which might have been expected to be present. That some facts of geographical distribution supplement other difficulties. That the objections drawn from the physiological difference between "species" and "races" still exists unrefuted. That there are many remarkable phenomena in organic forms upon which natural selection throws no light whatever, but the explanation of which, if they could be attained, might throw light upon specific origination.

It is, as we have said, out of the question our endeavouring to give place to quotations from the volume; our space does not permit it. We may observe, however, that Mr. Mivart, as we have already stated, proves, we think, decidedly that specific differences may be developed suddenly, and in many, if not all, cases are so developed. Admitting this, we can go no further. He has raised difficulties; he has produced numberless cases which are against the doctrine laid down by Mr. Darwin, but he has not given any in support of Mr. Darwin's views, and these we know to be infinite in number. He quotes Sir W. Thomson in support of the earth's age, but many persons are opposed to these views, which are at the best extremely hypothetical. Taking a difficult line of argument—and there are none more so than Mr. Darwin's —he asks why the geological record is not more popular; forgetting, as it seems to us, that the geologic facts must always be of the most rudimentary character, seeing that whole rock-formations may have been formed and washed away again, and indeed have been so. Mr. Mivart is, it seems to us, a little too much in favour of his own theory, and has not given Mr. Darwin's any of the immense arguments in its favour. This, however, has been done unwittingly, for all through the volume there is the best of good feeling shown. Indeed, it is the only book we have ever seen where a purely controversial question is treated so fully and so fairly, and our best thanks are due to the author on this account. We cannot admit all that he desires to prove, but it appears to us that the book is worthy of a high rank from its having proved one point, and that a most important one. It is a volume which deserves to be read by every naturalist, and one which will continue to live long after its author has ceased to exist.

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*The Student's Guide to the Practice of Measuring and Valuing Artificers' Works,* by E. W. Tarn, M.A., Architect. New edition. London: Lockwood, 1871.—This is a valuable work for all who desire a standard guide to the methods employed by surveyors in their measurement of builders' works. The several rules and plans it contains appear good and clear. Extending over more than 300 pages, it necessarily deals largely with the subject, and is so copiously illustrated that we think the student need have little difficulty in mastering it. We wish our space would permit us a longer notice, for the book really deserves it.

*Metallography as a Separate Science, &c.*, by T. Allen Blyth, M.A., Ph.D. London: Longmans, 1871.—Dr. Blyth seems as fond of poetry as of metallography, and uses it largely in the book. We do not see the force of his arguments in favour of making this a separate science from chemistry, nor do we see anything new that is valuable in his pages. The book contains some facts not stated in most chemical works, but they are very few. We do not approve of the volume *tout entier*.

*Atchley's Civil Engineers' and Contractors' Estimate and Price-book for Home or Foreign Service*, by W. D. Haskoll, C.E. London: Lockwood, 1871.—This book will be found useful by all who are engaged in building, &c. It contains, arranged in tables, the costs of every conceivable work connected with building, fencing, draining, road-making, &c. It seems a good book.

*On the Aymara Indians of Bolivia and Peru*, by David Forbes, F.R.S., F.G.S., &c. London: Taylor & Francis.—Mr. Forbes has here reprinted for private circulation his admirable essay, read before the Ethnological Society in June last. It contains 100 pages, six page-plates, most admirably executed, and a long list—a sort of dictionary—of the words in the language of the people. The book is most interesting, but it is especially valuable because of the measurements of bones of this peculiar people which it contains, and which we believe have been considered very valuable by Mr. Darwin.

*The Elements of Algebra and Trigonometry*, by W. M. Griffin, B.D. London: Longmans, 1871.—This is the latest of Messrs. Longmans' admirable series of scientific works. It seems to be a well-arranged manual, containing abundant examples, and leading the student on fairly. It is, of course, intended for artisans and others, but we fear for few of the former.

*Aunt Rachel's Letters about Water and Air*. London: Longmans, 1871.—Is a simple plain account of the leading phenomena in the branches of natural philosophy taken up. We can recommend it for young boys and girls.

*The Aboriginal Tribes of the Nilgiri Hills*, by Lieut.-Col. W. Ross King, F.R.G.S. London: Longmans, 1871.—These singular people are very well described in a paper read by Col. King before the Anthropological Society, and reprinted here. The paper is a most interesting one, and will repay perusal.

*The Modes of Dying and the means of obviating the tendency to Death*, by W. F. Cleveland, M.D. London: Churchill, 1871.—This is Dr. Cleveland's annual address to the Harweian Society. Without containing anything absolutely new, it is a forcible address and contains some very curious cases. We think the author is to be thanked for pointing out methods not new, but too unknown, for prolonging and saving life.

*Manual of the Science of Colour*, by William Benson, Architect. London: Chapman and Hall, 1871.—This is really a book which should be in the hands of every artist, and of all who have to do with colour. It is to our minds the only accurate view which has been put forward. We have passed this opinion before, and we hope that the book will be appreciated.

*Odd Showers, or an Explanation of the Rain, &c.*, by Carribber. London: Kerby & Son, 1870.—A little book which tells of the following showers: insects, fishes, soot, sand, and ashes; sleet, rain, snow and meteorites. It is a useful and very small work.

*The Year Book of Facts in Science and Art*, by John Timbs. London: Lockwood, 1871.—This little book contains about the usual number of mistakes and errors. It opens with a fair engraving of Professor Huxley.

*Everybody's Year Book for* 1871 is Messrs. Wyman's sixpenny annual. It is a wonderful book, containing a little of almost everything and very well arranged.

We have also received *The Typhoid Fever in Islington traced to the Use of Impure Milk*, by Edward Ballard, M.D. London: Churchill, 1871. *The Astral Hebrew Alphabet*, London: Mackintosh, 1871. *The Correlation of Zymotic Diseases*, by A. Wolff, F.R.C.S. London: J. A. Churchill, 1871. *Letters on Vaccination*, by William Woodward, M.D. Worcester: Deighton and Son, 1871; and also a series of Reports of Patents and Abstracts of Patents relating to the *Preservation of Food*, by W. W. Archer, Registrar-General of Victoria, from Melbourne, Australia.

*The Descent of Man, and Selection in Relation to Sex.*—By Charles Darwin, M.A., F.R.S. 2 vols. London: John Murray, 1871.—We regret that this volume came into our hands too late for a notice in this number. It is a work of great importance, and one which could not be hastily reviewed. We notice its arrival, however, for the benefit of our readers, and in acknowledgment of the book from the publisher.

# SCIENTIFIC SUMMARY.

## ASTRONOMY.

THE *Total Solar Eclipse of December* 22.—At length the question of the corona—at least that great general question which had for the last few months been so earnestly discussed—is disposed of. It seemed, indeed, when the first intelligence came from the eclipse parties as though it would still remain a moot point whether the corona is in large part a terrestrial phenomenon, as Mr. Lockyer has urged, or whether not only the inner and brighter portion, but the outer radiated and fainter parts, belong to a true solar appendage. From Oran we heard of complete failure—a failure the more disappointing because so many of our most eminent men of science, as well as Janssen, the French spectroscopist, had betaken themselves thither. From Spain came news of the confirmation of the American observations of 1869, and, vaguely, of successful photographic operations—but nothing which promised to decide the questions at issue. From Syracuse we heard but of "substantial" results, which a long experience has taught us to regard as meaning—all but total failure. No one would have supposed, to judge from the meagre telegrams which alone reached us from Syracuse, that a brilliant success had rewarded the section under Mr. Brothers' charge. So rapidly had telegraphic news come to us when nothing important had to be told, that everyone was prepared to hear of the complete failure of those photographic operations from which so much had been expected. In fact, letters from Mr. Brothers himself first announced a success, which, in reality, is the distinguishing feature of the eclipse operations.

Comparing the accounts first received, there seemed, as we have said, no promise of a decision. At the meeting of the Royal Astronomical Society on January 13, Lord Lindsay's photographs were exhibited, and they seemed unsuited to decide the questions at issue. The accounts of Lieut. Brown, Mr. Hudson, M.A. (Fellow of St. John's College, Cambridge), and of others, were read, and only one matter referred to seemed to suggest the possibility that a decisive answer might be given to those questions. It was mentioned that at three stations, separated by spaces of about six miles, observers had noticed, opposite the south-eastern quadrant of the moon, a well-marked V-shaped gap. This was pictured in a fine drawing by Lieut. Brown, who mentioned that underneath the gap—that is, between the apex of the V and the corona—the bright inner portion of the corona was shallower than elsewhere—a most important observation if confirmed (as we shall presently see

that it was). But as yet we had no proof that Lieut. Brown's drawing, excellent as it was in itself, was more trustworthy in a scientific sense than those many wild pictures of total eclipses which have caused ere now so much perplexity.

Presently came a paper from Mr. Lockyer (who had unfortunately not been favoured with a view of the eclipse), summing up the evidence which had reached him—seemingly in a very imperfect form. His conclusion was that the inner and brighter part of the corona certainly belongs to the sun, the outer fainter and radiated portion being, he judged, as certainly atmospheric. He founded this opinion on the difference which could be discerned between the various pictures of the coronal radiations.

But the end was not yet. A photograph taken by Mr. Willard (an American observer), in Spain, had been found to show the great V-shaped gap which has been already referred to as seen by observing parties at widely-separated Spanish stations; and by about this time the tardy news of Mr. Brothers' success had reached his friends in England, who hastened to announce it as publicly as possible. He had taken six photographs, and in the fifth "the corona is seen," he wrote, "as it was never shown on glass before." Would the V-shaped gap be there? This was the thought which occurred to all who understood the position of the *vexata questio*. At length copies of his negative were sent to astronomers; and there the great gap was seen—unmistakably the most remarkable feature of the picture. Two other rifts, fainter and not reaching so far towards the sun's limb, were well shown; and on a reference to the American photograph it was found that there also, though less clearly, the place of these rifts could be discerned. In Lieut. Brown's picture, and in a picture taken by Professor Watson in Sicily, the corresponding depressions of the inner and brighter part of the corona were clearly indicated in corresponding situations. When it is added that in Mr. Brothers' photograph the radiations extend on one side to more than half a degree from the place of the eclipsed sun, and on the other nearly a degree, the decisive character of the evidence this noble picture has given will be immediately recognised.

It would seem, however, that an imperfect drawing of this photograph had been sent to Mr. Lockyer, who found the place of the great gap—as determined by two prominences—not strictly coincident with the place it occupies in the American photograph. Comparing this imperfect drawing with the photograph, in company with the American professors (Young and Winlock), he came to the conclusion (from which they did not wholly at that time dissent) that the gaps photographed by Mr. Willard and Mr. Brothers were different phenomena. Accordingly, in a second paper on the eclipse, he renewed the statement that the outer part of the corona is terrestrial.

But already photographic copies of the two negatives had been made to a common scale. Only a day or so afterwards, Professor Winlock examined such copies in company with Dr. Huggins, and expressed the opinion that the two great gaps are certainly the same in the American photograph and Mr. Brothers'—the two fainter ones probably so, but too faint for certainty. Sir John Herschel received similar copies, and in a letter read by Mr. Brothers at the meeting of the Royal Astronomical Society on March 10, the veteran astronomer expressed his conviction that the coincidence of so

well-marked a feature disposed finally of the terrestrial theory. Dr. Balfour Stewart, in a letter read at the same meeting, pointed to the decisive nature of the evidence afforded by the darkness of the moon in Mr. Brothers' photograph, mentioning, also (as Mr. Proctor had already done in a paper read a year before *), that on *à priori* grounds the atmospheric explanation of the radiations was untenable, since the proportion which atmospheric glare due to the inner corona would bear to the corona itself would be the same, or nearly so, as the proportion which the atmospheric glare in full sunlight bears to that sunlight—i.e. would be exceedingly small. In fact, at this meeting of the Royal Astronomical Society one piece of evidence after another was brought to bear against the unfortunate "atmospheric glare" theory, in whose favour not one of those present seemed ready to venture a word.

*The Results of the Eclipse Expeditions.*—Freed thus from the incubus of an erroneous theory which had too long been suffered to attract attention, let us consider the real results of the late expeditions. One can hardly speak of the proof that the corona has a real existence, and is a real solar appendage, as a result of the late expeditions, because it had, in effect, been demonstrated much earlier; though undoubtedly the acquisition of evidence easily interpretable and generally convincing must be regarded as a gain. But the student of science has a right to inquire what fresh knowledge has been acquired. It appears to us that by far the most important result of the expeditions is to be found in the evidence which the photographs give as to the structure of the corona. We have seen that Lieut. Brown had noticed that the inner and brighter part of the corona is much shallower where the great gap appears, and that Professor Watson's drawings confirm this. But the evidence of the American photographs and Mr. Brothers's is decisive on the point. The correspondence between the outer radiations and the inner brighter portion of the corona is unmistakable. This is a phenomenon worthy of the most careful study. It can only be explained—*nobis judicibus*—as due to the action of solar forces exerted radially; but whether those forces be eruptive, or simply repulsive, is not so clear. The action of eruptive forces sufficing to account for the observed extension of the corona may seem at first to involve an incredible degree of activity beneath the solar photosphere; but when it is mentioned that a velocity only three times as great as that with which the hydrogen of the prominences is observed to be flung out (after passing through the denser lower regions of the solar air) would suffice to project matter as far as our own earth, while a very

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* He thus wrote ("Monthly Notices" for March 1870): "We know how small a relation ordinary atmospheric glare bears to direct sunlight, and the glare due to the prominences and chromosphere" (an objectionable word—he should have written *sierra*) "would bear a similar relation to the direct light from those sources." He added that the light from this source "would extend over the moon's disc, since it would illuminate the air between the observer and the moon's body." The fact that such light was recognised by the spectroscopists during the late eclipse—the bright line coronal spectrum being actually discernible when the spectroscope was directed to the middle of the moon's disc, shows how, by careful reasoning, the results of observation may be anticipated.

small relative increase of velocity could carry matter away from the sun *never to return*, this objection will not appear decisive. We might thus, also (by extending similar considerations to the stars), find an explanation of those facts on the strength of which Mr. Stanislas Meunier has adopted the belief that meteorites are *astral* in origin, as well as those yet more remarkable facts revealed by Mr. Sorby's microscopic, and the late Professor Graham's chemical, analysis of these bodies. The great velocity of some meteors, or (which is the same thing) the hyperbolic figure of the paths on which they approach the sun—a phenomenon hitherto deemed inexplicable—would be accounted for also in this manner. But whatever theory we adopt to interpret the matter, it remains as a demonstrated fact that the corona indicates the action of radial solar forces.

Amongst other results of the eclipse expeditions must be mentioned the important observation made by Professor Young and Mr. Pye, of the reversal of all the Fraünhofer lines close by the sun's limb at the instant of totality and for a few seconds after. This proves that there is close by and outside the visible photosphere a dense and highly complex atmosphere. We may say, in fact, that Professor Young and Mr. Pye have determined the birthplace of the Fraünhofer lines. Mr. Lockyer, indeed, has expressed doubts whether Professor Young and Mr. Pye could have seen the Fraünhofer lines thus reversed; because he considers that the method by which the limb of the uneclipsed sun is spectroscopically observed ought uniformly to show the reversal, whereas he has observed it but once and Professor Young never. He has omitted to consider, however, that in one important—or, rather, essential—respect, an observation made at the moment of totality, when the solar and lunar limbs touch, must have an enormous advantage over one made by the Janssen method. The effects of irradiation, and of the sensible dimensions of the optical image of each point of the solar photosphere, would in the latter case more than suffice to obliterate all traces of the complex atmosphere, even though that atmosphere were a hundred miles or so deep (as was pointed out in Mr. Proctor's papers in the last number but one of this magazine *). But when the limb is viewed as by Young and Pye both these effects are completely got rid of. It may be added that Father Secchi's observation of a continuous spectrum at the very limb of the uneclipsed sun is confirmed, as Professor Young points out, by the reversal of the Fraünhofer lines seen by the latter and Mr. Pye.

Great doubt rests on the polariscopic observations, insomuch that while some are assured that the light of the corona was radially polarized, others are equally positive that it was either polarized in the same plane as the atmospheric light, or not at all. The spectroscopic observations prove that the light of the corona may be divided into two chief portions—one giving a faint continuous spectrum (probably this is the largest portion of the corona's

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* We are requested to state that in the last line but one of p. 40 in our last number, Mr. Proctor has, by mistake, written "the honorary secretary" for "the assistant honorary secretary, Mr. A. C. Ranyard." It was due to the courteous letters of this gentleman that some who took part in the expeditions were prevented from withdrawing on the score of rather abrupt treatment.

light, notwithstanding its faintness when dispersed), the other giving a spectrum of bright lines, of which far the brightest is either the line "1474" of Kirchhoff's scale, or very close indeed to it. This line is seen in the spectrum of iron, and appears in the spectrum of the aurora. Professor Young had long since mentioned reasons for believing that it belongs to a new element, and this view has lately been adopted by Mr. Lockyer.

On the strength of observations made during the eclipse Father Secchi expresses the assurance that the chromosphere is not a solar atmosphere, but made up of many relatively minute prominences.

*Sun-Spot Observations at Kew.*—The following summary of sun-spot observations made at Kew has been communicated to the Royal Astronomical Society by Messrs. De la Rue, Stewart, and Loewy.

| Months. | Days of observation. | Days without spots. | Number of new groups. |
|---|---|---|---|
| January | 11 | 0 | 17 |
| February | 12 | 0 | 26 |
| March | 14 | 0 | 31 |
| April | 22 | 0 | 31 |
| May | 25 | 0 | 40 |
| June | 19 | 0 | 39 |
| July | 18 | 0 | 36 |
| August | 25 | 0 | 44 |
| September | 21 | 0 | 31 |
| October | 18 | 0 | 39 |
| November | 17 | 0 | 22 |
| December | 11 | 0 | 37 |
| Total | 213 | 0 | 403 |

They remarked that the year 1870 "was characterised by an exuberance of solar energy which is without parallel since the beginning of systematic observations (i.e. since 1825). The number of observed groups far exceeds that in any previous year; and it appears also from a cursory comparison with the maximum years' observations, as recorded by Hofrath Schwabe, that the magnitude of the different groups, as well as the average amount of spotted surface during any period of the year, is unprecedented." As the latter half of the year shows an increase of no less than thirty-five groups over the first half it seems likely that the maximum has not yet been passed, even if it has been reached. "A very remarkable feature of the groups observed during the year," adds the report, "appears to be their remarkable lifetime. . . . There can be no doubt, from the observations, that an exceedingly large number of groups completed three, four, and even more revolutions before finally collapsing. Whether this peculiarity in the behaviour of groups belongs to all maximum years—whether the groups in minimum years are, on the whole, of a more ephemeral existence—and, further, in what manner the duration of any single group is connected with or dependent on its magnitude and the law of periodicity, are questions very forcibly suggested by the observations of the past year."

*Notes on the Floor of Plato.*—Mr. Birt continues his collection of observations on the floor of Plato. He considers it not impossible that the

results may be explained as due to a present lunar activity; but he adds, very prudently, "that it is desirable observations should be multiplied, especially as there are strong objections to such a view, though the idea has been mooted during the last eighty years."

*Supposed New Variable in Orion.*—Mr. Webb calls attention to a red star in Orion which he considers to be also (like many other red stars, by the way) a variable star. He says: "I have at present no means of giving its place with due accuracy, but it is easily found about 6m. 18s. in time west of 42 Orionis, and nearly on the parallel of a minute open pair, about 11 magnitude, 9′ or 10′ north of that star.

*Periodical Changes in the Physical Condition of Jupiter.*—Mr. A. C. Ranyard notes evidence in favour of Mr. Browning's view that the recent changes in the appearance of Jupiter may be associated with those solar disturbances which have recently been so remarkable. "A similar increase of colour and bright egg-shaped markings" in the great equatorial belt, "were observed," he remarks, "in the years 1858, 1859, and 1860. Mr. Huggins, Mr. Airy, and Sir W. K. Murray all noticed and figured them, their drawings, in many respects, corresponding with those made in the course of last season." In 1860 the sun showed many spots. In 1850, when the sun was also much spotted, Jupiter was similarly disturbed. Mr. Ranyard also quotes earlier instances. He notices "a most interesting remark of Cassini's," which relates, however, to a well-known peculiarity of Jupiter's spots. Cassini "observed that the bright markings upon Jupiter had a proper motion of their own, and that that motion was greater the nearer the spots were situated to Jupiter's equator." This has been often noticed since; but perhaps the most remarkable known instance of this excess of motion near the equator is the case of the dark rift seen across a bright belt for six weeks in succession in the spring of 1860. The equatorial or southern end of this rift travelled away from the northern end at the rate of about 190 miles per hour! This rapid proper motion of one end of a vast rift in a cloud-belt—to say nothing of the persistence of the rift for at least 100 rotations of the planet (that is, by day and by night for 100 Jovian days)—surely disposes effectually of the theory that the cloud-belts of Jupiter are raised by solar action resembling that to which our own cloud-regions are due. Mr. Ranyard closes his paper with the remark that "if a future more complete examination of the observations of Jupiter should confirm the suspicion that the sun and Jupiter have the same period of maximum disturbance, it would appear to show that the alternations on Jupiter are dependent upon some cosmical change, and not on any effect of tides, as suggested by Dr. Wolf in the case of the sun." Is it altogether so clear, however, that the imagined action of Jupiter in raising solar tides could not synchronise with a solar action raising tides in the deep Jovian atmosphere? We say this not as advocating the tide theory, but to show that the mere coincidence of solar and Jovian disturbances in point of time does not necessarily prove that the disturbing cause is cosmical as distinguished from some form of action exerted by these two bodies upon each other.

*Common Proper Motion of the Stars 36 (A) Ophiuchi and 30 Scorpii.*—Mr. Lynn has recalculated the remarkable proper motion common to both

these stars, which lie about 12½ minutes of arc from each other. He finds that the annual proper motion of each star deducible from the Greenwich observations is:—

| | In R. A. | In N. P. D. |
|---|---|---|
| $A^1$ Ophiuchi | $-0^s{\cdot}029$ | $+1''{\cdot}20$ |
| $A^2$ Ophiuchi | $-0{\cdot}043$ | $+1{\cdot}05$ |
| 30 Scorpii | $-0{\cdot}042$ | $+1{\cdot}17$ |

No doubt can remain that we have here a drifting system.

*Proper Motion of Oeltzen's Argelander* 17,415–6.—Mr. Lynn estimates for the proper motion of this ninth magnitude star

in R. A. $-0^s{\cdot}07$
in N. P. D. $+1''{\cdot}20$

*Memoir by Hansen on the Transit of Venus.*—Dr. Hansen deals chiefly in this paper with the possibility of obtaining the solar parallax by simple measurements of the distance of Venus from the sun's centre at the moment of greatest phase. In the last of three appendices he exhibits formulæ "for the application of photographic observations to determine the parallax. The formulæ require (besides the distance of the centres of the sun and Venus) the determination of the position-angles of Venus at the two stations respectively. It is suggested that there should be at the focus of the instrument a wire in the plane of the declination circle to be represented in the photograph, and thus to serve as a ground-line for the measurement of the position-angle;" but Dr. Hansen is unable to judge of the degree of accuracy with which the position-angle can be thus determined. The difficulty disappears when observations are made upon the plan proposed by Mr. Proctor, for by his method only estimates of distance between the centres of Venus and the sun would have to be considered.

*Nautical Almanac for* 1874, *and the Transit of Venus.*—In the recently issued "Nautical Almanac for 1874" the phenomena of the transit of 1874 are calculated for nearly all the suitable observing stations. Probably the tables dealing with these phenomena will be regarded as disposing finally of any doubts which may have been entertained respecting the accuracy of Mr. Proctor's statements where these differed from the statements of the Astronomer Royal. For example, it was somewhat confidently stated that at Nertchinsk—which Mr. Proctor proposed as a suitable northern station for applying Halley's method—the sun would not have both at ingress and degress of Venus anything like the elevation of 10° which had been pronounced necessary for effective observation; whereas Mr. Proctor asserted that at both internal contacts the sun would have a sufficient elevation. The "Nautical Almanac" gives for internal contact at ingress a solar elevation of 12°, and for internal contact at egress a solar elevation of 10°. In fact, so far as the "Nautical Almanac" tables extend, they confirm every one of Mr. Proctor's statements in all essential points—differing only as to small details depending on the selection of the value of Venus's semi-diameter and the sun's.

*Planets for the Quarter.*—Jupiter's distance is gradually increasing, but he continues to be an evening star, and not unfavourably situated for observation during the earlier part of the quarter. Saturn will be in opposition

on June 28, but will be too low for favourable observation. Mars is the planet of the quarter. He was in opposition on March 19, and will continue for several weeks favourably placed, both as respects distance and altitude. His northern hemisphere is now considerably bowed towards us and towards the sun; and as this hemisphere has been less completely studied than the southern, observers who have good telescopes would be doing good service in studying the planet. Although the oppositions which occur when Mars is, as now, near his aphelion, are less favourable as respects distance, the planet is more favourably situated as respects altitude. Venus is coming round into a better position for observation, and towards the end of the quarter will be nearly at her brightest. It is satisfactory to know that astronomers propose to study her with special care during the approaching months; so that perhaps some of the perplexing questions which have been suggested by her spots, rotation period, inclination, &c. may be satisfactorily dealt with.

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## BOTANY AND VEGETABLE PHYSIOLOGY.

*Experiments on the Influence of Manures on Plants.*—Some very important experiments in this most interesting direction have been carried out by Dr. Masters, F.R.S., and Dr. Gilbert, F.R.S., and reported upon to the Horticultural Society, which caused them to be undertaken. The experiments were, as might be imagined, partly failures, for it is natural that mistakes should be made at the first; but they are of special interest nevertheless, and we have no doubt that from the next ones we shall have much valuable matter to learn. One of the tables furnished by the reporters shows us that the *grasses* removed from 0·5 to 1·23 oz., or an average of about 0·8 oz., of mineral matter from the unmanured soil; whilst under the same conditions the *Leguminosæ* removed from under 1 to over 2 oz.; the *Achillea* 3¼, and the *Plantago* nearly 3½ ozs. Some idea of the richness of the soil in available mineral matter which these amounts indicate will be acquired when it is stated that 1 oz. of mineral matter removed from one of the boxes by one of the grasses would correspond to as much per acre as would be contained in about 5 tons of meadow hay, 1 oz. in one of the clovers to as much per acre as would be removed in about 4 tons of clover hay, and 3 to 3½ ozs., as in the cases of the *Achillea* and *Plantago*, to a ton or more of mineral matter per acre. Of *phosphoric acid*, the *grasses* would, on the average, thus remove from the unmanured soil about one-seventh as much as was supplied where the heavy mineral manure was employed; and of *potass* from one-fourth to one-third as much as the mineral manure supplied. Of *phosphoric acid*, the *clovers* would remove considerably more than, and of *potass* about as much as, the average of the grasses. Of both constituents the *Lotus* would remove more still, and the *Achillea* and *Plantago* probably very much more. With nitrogenous, but without mineral manure, the amounts removed were, as a rule, with all the plants much greater than—in fact, in some cases once and a half as much as—without manure. With regard to *mineral constituents*, therefore, it may be concluded that the unmanured soils were so far drawn upon by the first year's growth as to widen considerably the difference

between the unmanured and manured conditions; and hence the series of soils would be better fitted for the purposes of further experiment.

*American Experiments on the Compass Plant.*—Mr. Thomas Meehan has laid a series of remarks before the Academy of Sciences of Philadelphia (October) upon the Compass plant, which clear up the question as to its pointing to the north or not, which many people deny. When first he saw the *Silphium*, to any great extent, in its natural localities, there was not the slightest indication of this northern tendency. It was a great surprise, as a limited knowledge of it before had taught the reverse. He determined to watch a plant carefully on his own grounds the next year. The result was just as described by President Hill, and related in our last number of "Popular Science Review." There was the unmistakable northern tendency in the leaves when they first came up, and until they were large and heavy, when winds and rains bore them in different directions, and they evidently had not the power of regaining the points lost. This often took place by their own weight alone, especially in luxuriant specimens. Mr. Hill says it was in June when he saw them on the prairies, all bearing north; when Mr. Meehan saw them, and not doing so, it was early in September, and then no doubt the mechanical causes he has referred to had been in operation. The plant he has had in his garden now for some years affords much interest in many respects. He learned a useful lesson from it this year, in reference to the relative rates of growth in the different parts of the inflorescence. Noticing that there appeared to be no growth in the disc florets in the day, he determined to note accurately one morning during the last week in August exactly when growth did commence. The ray flowers close over the disc during night, and at 4 A.M., with day just dawning in the east, he found the ray petals just commencing to open back. In the disc there are about fifteen coils of florets in the spiral.

*The Plants of West Newfoundland.*—A recent number of the "Canadian Naturalist" contains an able paper on the above, by Dr. John Bell. Its length and the discursive habit of the author prevent us giving an abstract of it.

*The Herbaria of Linné and Michaux.*—Professor D. C. Eaton, M.A., of Yale College, U.S., the eminent American pteridologist, when in Europe on a visit in 1866, examined many of the standard herbaria, and made notes on the American plants contained in them. He has most liberally placed a series of these notes on the North-American Filices in Mr. D. A. Watt's hands for perusal, has allowed him to take copies of them, and to print such selections from them as he might deem of sufficient interest: those relating to the collections of Linné, now in London, and of Michaux, in Paris, are given. The herbarium name of each plant is placed within quotation marks, as are also such notes (of habitat, &c.) as were deemed of sufficient interest to be copied from the sheets to which the respective specimens were attached. Mr. Eaton's observations follow. He has not printed these *verbatim*, as, not being intended for publication, they were, more or less, made up of indications and signs which he has attempted to write out with exactness. One or two observations of his own are placed within brackets, and bear his initial. For convenience of reference he has arranged the species in the order of their occurrence in the species Plantarum, and in

the Flora Boreali-Americana. The notes of Mr. Watt's are of especial interest, but are too long for insertion here.

*To those interested in the Laminariaceæ.*—It has been recently stated by Dr. Lawson that Dr. A. F. Le Jolis, of Cherbourg, France, is engaged in a monograph of the whole group of the *Laminariaceæ*, that for such a study materials are never too numerous, and that he would be happy to receive a fresh supply of specimens from North America. He asks Dr. Lawson's help, and that he would interest his friends in his favour. It is not necessary that the specimens be prepared for the herbarium. On the contrary, he had rather they were coarsely dried, without being washed in fresh water or compressed. The parcels may be addressed to him, and sent by any vessel to France.

*The Vitality of Yeast.*—Mr. H. J. Slack, in his recent interesting and instructive address to the Royal Microscopical Society, stated that M. Melsens made experiments last year on the vitality of beer-yeast. He found fermentation possible in the midst of melting ice, a temperature at which the yeast would not germinate. The life of the yeast-plant was not destroyed by the most intense cold that could be produced, about 100° C. below zero. In close vessels when the products of fermentation gave a pressure of about twenty-five atmospheres the process stopped, and the plant was killed. M. Boussingault, who was present when this communication was made to the French Academy, accepted the statement, on account of the known ability of M. Melsens, but he detailed experiments to show that other ferments had their activity destroyed by exposure to temperatures much less severe, or even by ordinary frost.

*Dr. Gray, of America, and Professor S. B. Buckley* have had some considerable disputation before the Philadelphia Academy (see "Proceedings," December 1870) on the supposed new plants from Texas. Dr. Gray has differed from Professor Buckley materially; consequently the latter has come forward to justify himself. It seems to us, from a cursory glance at his paper, that in some of his assertions Professor Buckley is correct, but that in by far the greater number undoubtedly Dr. Gray has at present the advantage. Professor Buckley states, what we must deplore very much, that at the time of the American war "a large collection of rare plants" which he had made during 1859, '60, and '61, in Georgia, Alabama, Mississippi, Louisiana, and Texas, which he had boxed and started with for the North prior to the war, were stopped and destroyed at Lavaca, Texas. They were intended for, and directed to, the Academy of Natural Sciences of Philadelphia.

*A "Find" of Diatoms in the Sea.*—Mr. E. Bicknell, of the Museum of Comparative Zoology at Cambridge, Mass., exhibited to the American Association at their late meeting some diatoms recently thrown up by the sea at Marblehead, Mass. The deposit first found belonged to brackish water, as indicated by the nature of the diatoms and the presence of fruit of the *Characeæ*. The second deposit occurred about a mile from the first, and was purely of fresh-water origin; consisting of peat with fresh-water diatoms—*Pinnularia, Stauroneis, Navicula rhomboides, N. serians*, &c. These deposits were thrown up by a severe storm on March 31 last, and are believed to be the first fresh-water or brackish deposits known to exist

under the present ocean. They seem to be conclusive proof of the recent encroachments of the ocean upon the shore-line in that vicinity.

*An Abnormal Potato, one growing from the centre of another*, was some time since presented to the Philadelphian Academy and was reported on by Mr. T. Meehan. It had been handed to him by the curators, and on dissection, though no exact place of origin could be traced, there seemed nothing to indicate any other theory of origin than that one potato had really grown out of the centre of the other. But there were serious physiological reasons in the way of such a theory. A potato tuber is really but a thickened axis, in which the greater part of the interior structure would be incapable of developing a bud which would produce a tuber such as this one had done. The origin of a new tuber from an old one would be nearer the old one's surface. He had been looking for some further explanatory facts, and believed he had them then, in the potato tubers he handed to the members. They were about the size of hen eggs, and were pierced in every direction by stolons of the common couch grass, *Triticum repens*. They had gone completely through, as if they were so much wire, and in one instance two tubers had become strung together by the same stolon, as if they were two beads on a string. One would suppose that the apex of the stolon, when it came in contact with the hard surface of the tuber, would turn aside and rather follow the softer line of the earth; but there was no appearance of any inclination to depart from their direct course. They had gone apparently straight through. He had no doubt the potato before referred to was a similar case, a potato stolon had penetrated another potato, and instead of going through as these grass spears had done, terminated in the centre, and formed the new potato there. It was worthy of thought whether so much attention had been given to this direct force in plants as the subject deserved. It was well known that a mushroom would lift a paving-stone many times its own weight, rather than turn over and grow sideways, which it would appear so much easier for it to do; and tree roots growing against walls would throw immensely strong ones over, though one would think the pressure against the softer soil would give room for their development, without the necessity of their expending so much force against the wall.

*Culture of various Herbs and Plants.*—On October 10, M. Decaisne read a paper before the French Academy recommending strongly the advisability of cultivating various forms of herbs and plants during the siege. He points out that great results would be obtained and that the thing could easily be done.

*Structure of Ferns.*—The fact that Paris was besieged does not seem to have interfered much with the botanists. In the "Comptes Rendus" of the Academy for October 31, we find M. Trécul discussing the ferns as fully as ever. In regard to *Didymochlæna sinuosa* he cites all the old and recent authorities from whom he differs, and in this manner occupies very nearly nine pages of the Journal. The memoir must be read carefully by those interested; it is much too long for an abstract.

*The Development of the Leaves of Sarracenia.*—M. H. Baillon, in a memoir presented to the French Academy by M. Brongniart (November 7), enters very fully into this subject and discusses the views of Saint-Hilaire,

Duchartre, Dr. Hooker, and others. Botanists are not agreed as to the exact significance of the different parts of the leaf. The most generally accepted opinion is that of the two former botanists. The author has studied the development of the leaves, and comes to the following conclusion. At first the leaves are represented by little buds, little elevations with a convex surface. At a little later period the base of these organs dilates a little and becomes slightly concave inwards. This is the first rudiment of the sheath, a portion of the leaf which we see has no relation with the cavity of the urn of *Sarracenia.* This vagina, which will take later on a great development, bears itself here as in all the vegetables in which it is found, and has no influence upon the constitution of the urn. The first indication of the latter is a small depression, a sort of "*fossette,*" very slight at first, which produces itself gradually and within the cone which represents the young leaf. This depression is really due to an inequality of development in the various parts of the summit of the leaf. In this respect the leaves of *Sarracenia* behave themselves like those of the *Nymphæaceæ* with which they have other analogies. The remainder of M. Baillon's paper is, though short, too long for an abstract. It should be all translated to render it intelligible, but it is not without value as a paper on vegetable morphology, if we may use the term.

## CHEMISTRY.

*The Production of Acetic Acid by the Destructive Distillation of Resin.*—Mr. Charles R. C. Tichborne recently (January 9) read a paper on this subject before the Royal Irish Academy. He said he found that, when resin was submitted to distillation, among other products was a strongly acid solution. The silver salt of this acid was formed, and on analysis it proved to be acetic. He remarked that it was rather surprising to see acetic acid produced in appreciable quantities from a substance so comparatively poor in oxygen. The amount of oxygen in colophony was 10·6 per cent., whilst in an acid-yielding substance, such as woody fibre, it was 49·4 per cent.

*Amorphous Sulphur.*—A recent number of "Poggendorff's Annalen" (No. 11, 1870) contains a paper on this subject by Herr R. Weber. The paper contains the account of a series of experiments made with sulphur obtained by precipitation (by means of acids) from hyposulphites, alkaline sulphurets, and the decomposition of sulphuretted hydrogen. Sulphur obtained from hyposulphite of soda by the addition of some hydrochloric acid to the solution of that salt, exhibits the appearance of an oily fluid, which remains liquid for a considerable time after having been washed by carefully-conducted decantation, and resembles, as regards colour and consistency, the yolk of eggs. The sp. gr. of this sulphur varies from 1·92 to 1·927; it is amorphous, but becomes, when completely solidified, crystalline, a phenomenon which is greatly accelerated by heat. The author found that the liquid sulphur contains small quantities of persulphide of hydrogen, but the origin of that substance could not be traced. The assertion often made,

that crystalline sulphur by contact with acids is converted into amorphous sulphur, is not found to be correct when experiments were purposely instituted to test this.

*Amygdaline and an Asparagine-like substance in Vetch.*—Herren Ritthausen and Kreusler have contributed a paper on these substances to the "Journal für praktische Chemie" (No. 18, 1870), which is thus abstracted in the "Chemical News." The authors, while experimenting with vetch obtained from Attica (Greece), found, on treating the coarse powder of this seed with water, that it gave off the smell of hydrocyanic acid, which, on nearer investigation, was found to be due to an amorphous modification of amygdaline. As regards the presence of an asparagine-like substance, the authors found a crystalline body, which, on being submitted to elementary organic analysis, gave results leading to the formula $C_8H_{16}N_8O_6$; this substance is difficultly soluble in water, more readily so in boiling dilute alcohol, and almost insoluble in boiling alcohol at 85 per cent.

*The Chemistry of Compressed Leather.*—In "Dingler's Journal" for December Dr. Dingler states that offal of leather, cuttings, and scraps are first cleansed from dirt and dust, then soaked in water containing 1 per cent. of sulphuric acid, until the material becomes soft and plastic, next compressed into the shape of blocks, dried by steam, and lastly rolled out in mills. In order to soften the mass, 1 lb. of glycerine is added to 100 lbs. of material. The leather thus again obtained is applicable for the inner soles of boots, &c.

*Dr. Wagner's Chemical Technology and Gmelins' Work.*—The eighth edition of Dr. Wagner's well-known work on "Chemical Technology" is, according to the "Chemical News" of January 27, soon to appear. An English edition of this valuable work is in preparation, and will be published shortly after the German edition. From a short notice which appeared in the "Zeitschrift für Chemie," No. 21, we learn that Dr. K. Kraut has edited four of the hitherto unfinished parts of Gmelins' work, including an excellently arranged general index. The publisher is M. K. Winter, of Heidelberg.

*Decomposition of Sulphide of Carbon by Heat.*—In the "Journal für praktische Chemie" (No. 16) W. Stein relates a series of experiments made with perfectly pure sulphide of carbon. His results show that sulphide of carbon is not decomposed by a very high temperature if charcoal is simultaneously present; it is therefore necessary to keep the retorts plentifully supplied with either charcoal or coke.

*How to Estimdte the Total Carbon in Iron.*—In "Dingler's Journal" (first number, for November 1870), Dr. Wittstein first states that the method suggested for the estimation of carbon in iron (crude cast-iron) by the late M. Berzelius, is the best and most simple. It consists in treating the iron with chloride of copper. The author's experiments with this method were conducted as follows:—1·25 grms. of coarsely pulverised iron were added to a liquid contained in a flask, and consisting of 50 grms. of water, 10 grms. of chloride of sodium, and 10 grms. of sulphate of copper. The iron was left in this solution for a couple of days. Ten grms. of hydrochloric acid, sp. gr. 1·13, were then added. The flask was next heated on a sand-bath. By this operation the hydrated oxide of iron and the finely-divided metallic copper were dissolved, and after the liquid had been diluted with about twice its bulk of water, the carbonaceous matter was collected on a

previously weighed filter, and dried at 100°. The quantity obtained weighed 0·097 grm., losing by ignition a quantity of 0·042 grm., which amounts to 3·36 per cent. of carbon. The residue of the ignition was tested for the presence of iron and copper by dissolving it in nitro-hydrochloric acid (*aqua regia*). Both metals, to the amount of ·1 centigrm., were found to be present; the remaining silica was still found to contain a small quantity of carbon.—See also the "Chemical News."

*An Occasional Origin of Nitrates in Water.*—Mr. Charles Ekin has (Chemical Society, January 19) found considerable quantities of nitric acid in spring water, for which he could not account by supposing it to come from some sewage contamination. Closer examination showed that the water in question had passed through a fossiliferous stratum. This observation necessitates a modification of the "previous sewage contamination theory."

*A New Alkaloid from Cinchona Bark.*—Mr. D. Howard, in a paper read before the Chemical Society on January 20, said that, in experimenting upon impure crystallisations of salts of quinine obtained from the mother-liquors of the manufacture of sulphate of quinine, he has occasionally been perplexed by an unusual loss in re-crystallising, which the mechanically adhering mother-liquor did not seem to account for. A more careful examination of some of these substances shows that the cause, in some cases at least, is the presence of an alkaloid hitherto undescribed, the extreme solubility of the salts of which both distinguishes it at once from the cinchona alkaloids already known, and renders it very difficult to separate from the uncrystallisable quinoidin. The most convenient method of obtaining it is to purify the alkaloids contained in the mother-liquor from the re-crystallisation of such impure products as he has mentioned by solution in ether, and, after evaporation of the ether, to dissolve with oxalic acid in as small a quantity of water as possible, and to allow it to crystallise. The oxalate thus obtained may be purified by re-crystallisation from water with the addition of animal charcoal, but he has never been able to free it entirely from a yellow colour.

*Chair of Chemistry at the London Institution.*—Dr. Henry E. Armstrong has been appointed Professor of Chemistry, an office once held by Mr. W. R. Grove, Q.C., and subsequently by Mr. J. Alfred Wanklyn. Dr. Armstrong studied chemistry under Professors Hofmann, Frankland, and Kolbe, and has been associated with Dr. Frankland and the late Dr. Matthiessen in original researches.

*The Discovery of Chloralum.*—This does not appear to rest with Professor Gamgee, as was supposed at first. Mr. J. Carter Bell, writing to the "Chemical News" (February 3, 1871), says: "With regard to the much-vaunted 'Chloralum,' I see Professor Gamgee says, in his letter of January 13, 'The agent (chloralum) had never been thought of in therapeutics until last January;' again, in his letter of the 27th, 'since I first thought of the chloride as an antiseptic just a year ago.' In Ure's 'Dictionary,' 1863, Article 'Disinfectants,' chloride of aluminium is mentioned as an antiseptic; it says, 'Meat, if well packed, cleaned, and washed with a solution of chloride of aluminium, will keep three months.' After that I hardly think Professor Gamgee can lay claim to the discovery of the antiseptic and

therapeutic properties of chloride of aluminium." This is of course too clear, and we are surprised that a volume so well known should not have been previously consulted by Professor Gamgee.

*An Examination of the Doctrine of Atomicities* is the title of a paper originally read at the Troy Meeting of the American Association. It has since been reproduced by the "Chemical News," and it will, we think, repay perusal. It is not a paper which could be profitably abstracted, or we should attempt it for our readers.

*Escape of the Abbé Moigno and Injury to M. Ch. Girard.*—The "Chemical News" of February 24 states that it has just received a letter from the Abbé, dated Paris, February 15, 1871. From this it understands that the distinguished *savant* had a narrow escape during the bombardment. A shell exploded in his bedroom, and destroyed more than a thousand valuable books, but he escaped uninjured. *Les Mondes*, the publication of which was suspended last September, will reappear as soon as communications are open. M. Ch. Girard has, we regret to say, received serious injury from the fall of a shell, but our readers will be glad to hear that he is now convalescent.

*Making Malt without Germination.*—In a paper which appears in "Dingler's Journal" for January 1871, Dr. H. Fleck gives the detailed account of a series of experiments made by him with the view to ascertain how far it is possible to substitute for the ordinary process of malting the method of steeping the grain (barley or any other) desired to be converted into malt, in weak and dilute acids, to obtain thereby the same effect as produced by germination, and, moreover, in a far shorter period of time. It appears that, provisionally, the author has succeeded in his attempt, but is engaged in further experiments. Dilute nitric acid, containing 1 per cent. of acid, yields excellent results.

*Death of Dr. Muspratt.*—It is with much regret that we have to announce the death of Professor James Sheridan Muspratt, M.D., F.R.S.E., &c., which took place at West Derby, Liverpool, on February 3. The deceased was born at Dublin on March 8, 1821, and was the son of the well-known founder of extensive chemical works established near Liverpool. The Professor was a pupil of the late Mr. Graham, first at Anderson's University, Glasgow, and afterwards in London, and also studied under Baron von Liebig at Giessen. The deceased was the founder of a College of Chemistry at Liverpool, and was well and widely known in the scientific world by a variety of scientific publications.

*Detection of Sewage Matter in Water.*—Since the statements of Mr. Heisch this subject has attracted much attention. The best article upon it is that of Professor Frankland, which concludes as follows:—Potable water mixed with sewage, urine, albumen, and certain other matters, or brought into contact with animal charcoal, subsequently developes fungoid growths when small quantities of sugar are dissolved in them and they are exposed to a summer temperature. The germs of these organisms are present in the atmosphere, and every water contains them after a momentary contact with the air. The development of these germs cannot take place without the presence of phosphoric acid, or a phosphate or phosphorus in some form of combination. Water, however much contaminated, if free from phosphorus,

does not produce them. A German philosopher has said "ohne Phosphor kein Gedanke." The above experiments warrant the alteration of this dictum to "ohne Phosphor gar kein Leben."

## GEOLOGY AND PALÆONTOLOGY.

*The Labradorite Rocks of America* have been very fairly described in a long paper by Dr. T. Sterry Hunt, in the "Canadian Naturalist." These peculiar labradorite rocks, presenting a great similarity in mineralogical and lithological character, have now been observed in Essex County, New York, and through Canada, at intervals, from the shore of Lake Huron to the coast of Labrador. They are again met with in southern New Brunswick, in the Isle of Skye, in Norway, and in south-western Russia, and in nearly all of these localities are known to occur in contact with and apparently reposing, like a newer formation, upon the ancient Laurentian gneiss. Geikie, in his memoir on the geology of a part of Skye,* appears to include the norites or hypersthenites of that island with certain syenites and greenstones, which he describes as not intrusive, though eruptive after the manner of granites (loc. cit., p. 11–14). The hypersthenites are represented in his map as occurring to the west of Loch Slapin. Specimens in Dr. Hunt's possession from Loch Scavig, a little further west, and others in MacCulloch's collection from that vicinity, are, however, identical with the North-American norites, whose stratified character is undoubted. His paper is of considerable length, but may be referred to with advantage.

*Noteworthy Points in Italian Geology.*—According to Mr. J. C. Ward, writing in the "Geological Magazine" for January, these are briefly as follows:—1. The geological records date back only to Jurassic times, and there is no direct evidence of land over this area until late Secondary or early Tertiary. 2. The formation of Italy has been effected in a very simple manner, namely, by the upheaval of three consecutive marine formations into a long chain of mountains, and by the deposition round this long island of marine strata belonging to the Miocene and Pliocene periods, and their subsequent moderate upheaval. 3. The time through which this history carries us back divides itself into three separate periods as regards action from below. (1) A period of tranquillity, or slow depression, during which tranquil marine deposition was going on. (2) A period of vast internal force manifested in the form of upheaval of land, and formation of lofty mountains. (3) A period, not yet entirely over, of the same force manifested in an outward or volcanic form.

*Graphite in the American Laurentian.*—Writing on this subject some short time since, Dr. Dawson says the quantity of graphite in the Lower Laurentian series is enormous. In a recent visit to the township of Buckingham, on the Ottawa River, he examined a band of limestone believed to be a continuation of that described by Sir W. E. Logan as the Green Lake Limestone. It was estimated to amount, with some thin interstratified bands of

* "Quar. Jour. Geol. Soc.," xiv. p. 1.

gneiss, to a thickness of 600 feet or more, and was found to be filled with disseminated crystals of graphite and veins of the mineral to such an extent as to constitute in some places one-fourth of the whole; and making every allowance for the poorer portions, this band cannot contain in all a less vertical thickness of pure graphite than from 20 to 30 feet. In the adjoining township of Lochaber Sir W. E. Logan notices a band from 25 to 30 feet thick, reticulated with graphite veins to such an extent as to be mined with profit for the mineral.

*Crocodilian Remains in America.*—At the meeting of the Academy of National Sciences, Philadelphia (November 1) Professor Leidy remarked that he had recently received from Professor Hayden's expedition a collection of fossils, mostly consisting of remains of turtles and crocodiles. He had formerly expressed surprise at the absence of remains of the latter among the great profusion of remains of mammals and turtles in the Mauvaises Terres deposits of White River and the sands of the valley of the Niobrara River. He now felt some wonder at seeing so many crocodilian remains, apparently of cotemporaneous age with some of the latter. The reptilian remains are generally in a very fragmentary condition, and have been picked up from the surface of the country. Several undescribed species of turtles were recognisable, but these would be characterised at a later period. From among the crocodilian remains he had been able to obtain a large portion of those of a skull of *Crocodilus Elliotti*, indicated some time ago from a jaw fragment. The skull appears to have nearly the form of that of *C. vulgaris* and *C. biporcatus*. It is about a foot and a half in length. Teeth appear to have been absent at the extreme fore part of the jaw. Immediately behind their usual position the palate presents a deep pit at each side of the naso-palatine orifice. The jaw is deeply indented laterally, just back of the position of the fourth tooth, and a less indentation is situated back of the ninth tooth.

*Chair of Geology and Mineralogy in Edinburgh.*—Our readers are perhaps aware that some time ago Sir Roderick Murchison offered the munificent sum of 6,000*l.* for the endowment of a Chair of Geology and Mineralogy in the University of Edinburgh, on the understanding that the annual proceeds of this sum would be supplemented by a grant from Parliament. We may state that Government has consented to this proposal, and has agreed to recommend an annual grant of 200*l.* The University is said to be largely indebted for this desirable result to the earnest co-operation of its member, Dr. Lyon Playfair.

*Thermal Springs in Cambridgeshire.*—The Rev. O. Fisher writes in the "Geological Magazine" (January), in opposition to the notice of Mr. Harner, that there are such things as thermal springs. "To-day," says Mr. Fisher, "I went into a farmyard in this village, and found them laying up the manure in heaps, previous to carting it away upon the land. The manure was already hot and steaming when they removed it from the area of the yard, on which it lay two feet deep. There stands a pump in the centre of the yard; and I asked the farm-servant, who lives on the spot, whether the water was warm. 'Yes,' said he, 'almost as warm as new milk. And so is the water from the other well' (which stands on the edge of the yard). I fetched a thermometer, and found the water in the yard at

65° Fahr., that in the well on the edge of the yard at 54°, while the temperature of the air was 44°. Snow has been lying on the ground for five days, and disappeared only last night. In thawing it has gone into the farmyard well, and discoloured the water; else probably the temperature might have been higher, for the workman considered the water less warm than usual. In these wells the water stands at about twelve feet from the surface. They are fed by springs from the lower chalk, the water being held up by the gault. In such a country as this, the idea of thermal springs being fed by faults from below seems improbable, since, though there may be faults, it is scarcely possible that open fissures can exist in the soft clays of the district."

*The Red Pipe-stone Quarry* (America).—Dr. Hayden, in a work recently reviewed, gives the following admirable description of the above, which Mr. G. A. Lebour thinks may be of interest—as it unquestionably is—to every reader of "Hiawatha":—"On reaching the source of the Pipe-stone Creek, in the valley of which the pipe-stone bed is located, I was surprised to see how inconspicuous a place it is. . . . . A single glance at the red quartzites here assured me that these rocks were of the same age as those before mentioned at James and Vermilion Rivers, and at Sioux Falls. The layer of pipe-stone is about the lowest rock that can be seen. It rests upon a grey quartzite, and there are about five feet of the same grey quartzite above it, which has to be removed with great labour before the pipe-stone can be secured. . . . . The pipe-stone layer, as seen at this point, is about eleven inches in thickness, only about two inches and a quarter of which are used for manufacturing pipes and other ornaments. The remainder is too impure, slaty, fragile, &c. This rock possesses almost every colour and texture, from a light cream colour to a deep red, depending upon the amount of protoxide of iron. Some portions of it are soft, with a soapy feel, like steatite, others slaty, breaking into thin flakes, others mottled with red and grey. . . . . There are indications of an unusual amount of labour on the part of the Indians in former years to secure the precious material." It is remarkable that its age is not yet settled.

*The Physical Relations of the New Red Marl, Rhætic Beds, and Lower Lias.*—At the meeting of the Geological Society on January 11 Prof. A. C. Ramsay commenced by stating that there is a perfect physical gradation between the new red marl and the rhætic beds. He considered that the new red sandstone and marl were formed in inland waters, the latter in a salt lake, and regarded the abundance of oxide of iron in them as favourable to this view. The fossil footprints occurring in them were evidence that there was no tide in the water. The author maintained that the new red marl is more closely related to the rhætic, and even to the lias, than to the bunter; and in support of this opinion he cited both stratigraphical and palæontological evidence. He described what he regarded as the sequence of events during the accumulation of the later triassic deposits and the passage through the rhætic to the lias, and intimated that the same reasoning would apply to other British strata, especially some of those coloured red by oxide of iron, including the permian and the old red sandstone, and part of the Cambrian. The paper excited a very long discussion, which we have not space to record.

*Websterite at Brighton.*—Mr. S. G. Perceval, F.G.S., states that in last July he observed that a deposit of Websterite, subsulphate of alumina, had been cut into, in excavating for the new system of drainage in the Montpelier Road, opposite the south end of Vernon Terrace. It occurs at a depth of 16 feet from the surface of the road, beneath a ferruginous deposit of varying depth, which overlies the chalk on the summit of the hill, consisting of ochreous clay with occasional flint-breccia and masses of hæmatite iron ore in some instances mammillated and associated with crystals of selenite. The iron ore is occasionally friable and of a cindery appearance, containing in its cavities angular pieces of chalk and occasional groups of crystals of selenite. The deposit of Websterite is about three feet wide at its junction with the overlying ferruginous mass, narrowing as it descends, apparently occupying a fissure in the chalk, which has at some time been filled with clay, or has been formed by some decomposing action on the chalk, the chalk intruding occasionally into the vein of Websterite.

*Dr. Carpenter's Views Opposed.*—Mr. A. H. Green contributes a very able paper to the "Geological Magazine" (January 1870), in which he analyses Dr. Carpenter's argument. Can, he asks, then, the fauna of the sea on whose bed the chalk of to-day is forming be said, *on a broad view*, to be the same as the fauna whose remains are preserved in the chalk of Dover? He is not surprised that certain low forms should be common to the two, because the conditions under which such creatures live do not in all likelihood involve that struggle for existence to which specific change is probably due; they have ample space and ample sustenance for animals of their simple requirements. Some few forms, too, somewhat higher in the scale, seem to have lived on in "the dark unfathomed caves of ocean" but little affected by the round of changes that have so largely altered the dwellers on the upper world, though here it seems that the modern representatives are only generically allied, and not specifically identical, with the older forms, a point of the highest importance. But, leaving these cases out of the question, are the two faunas, *as a whole*, a bit alike? Take one simple instance. The older chalk swarms with ammonites, scaphites, baculites, and belemnites, all well-marked and typical forms, not one of which will be embedded in the chalk of to-day; and the old chalk has not yet furnished a single fragment of a marine mammal, many species of which will be preserved in the modern chalk. A palæontologist would readily point out any number of similar contrasts between the two faunas; but what he has said will, he thinks, make it clear why it is that he cannot understand how anyone can say we are living in the cretaceous epoch, unless he at the same time asserts that the age of a geological formation is to be determined from those beds only which are formed out of *Foraminifera*, and by the *Foraminifera* alone of the fossils contained in such beds.

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## MECHANICAL SCIENCE.

*The Tower Subway.*—This tunnel under the Thames, which as an engineering work was carried out with so much skill and success, rather threatens to prove commercially a less satisfactory undertaking. At all

events, it has been found necessary to abandon the mechanical working of the tunnel, and to use it as a simple footway. Whether, with its working expenses thus reduced to a minimum, it will repay its cost of construction, time will show. The traffic under the new arrangement has been considerable.

*Railway Tires.*—A very interesting discussion is being carried on as to whether the tires of railway carriages are or are not more liable to fracture in cold weather than at other times. It has long been a popular belief that iron was rendered either more brittle or weaker by cold. But the belief does not rest on any very well established basis. On the whole, the balance of experimental evidence seems to show that iron is not reduced in tensile strength nor weaker to resist impact when very cold than at ordinary temperatures. Some recent ingenious experiments of Dr. Joule, communicated to the Manchester Philosophical Society, are in accordance with this view; but engineers generally would be glad if Dr. Joule's experiments could be repeated on a larger scale. Probably enough certain qualities of iron are more affected by temperature than other qualities, and in a matter of so much importance to the lives of travellers it is very desirable that fuller information on this point should be obtained. Dr. Fairbairn points to the crude process of shrinking the tires on the wheels, and thus inducing an initial state of tension in the tire as the primary cause of fracture, the objection to the shrinking-on process being that the exact amount of stress induced in the process is unknown, and depends on the skill and attention of the workmen. We do not remember to have seen it suggested, that there may be some difference in the rate of expansion and contraction of the iron of the tire and of the body of the wheel with change of temperature, yet a difference may quite possibly exist. The iron of the tires and the wheel arms and nave is not of the same quality, and probably the crystalline arrangement differs. If there were a greater contraction of the tire than of the body of the wheel, this would explain the fact, alleged by railway managers, that more tires break in winter than summer, without requiring an assent to the supposed weakening of the iron. Or perhaps it may help to explain why steel tires enjoy a greater immunity from fracture in cold weather than iron ones. The same explanation will not hold in the case of rails, which also are stated to break more frequently in winter; but, in their case, the greater rigidity of the supports when frozen would seem fairly chargeable with part at all events of the injury.

*Stability of Ships.*—All those who are interested in the scientific problem presented in the calculation of the stability of ships should study the documents accompanying Mr. Childers' minute on the loss of the *Captain*. Perhaps the clearest statement of the conditions to be attended to in estimating the stability of a vessel will be found in a very interesting paper by Mr. C. W. Merrifield, F.R.S., in the "Annual of the Royal School of Naval Architecture," recently published.

*Adding Machine.*—A very simple and neat machine for mechanically adding up long columns of figures will be found described in "Engineering" for January 27. The instrument has been invented by Mr. Webb, of New York.

*New Marine Boiler.*—A new form of water-tube boiler has been invented

by Messrs. Howard, of Bedford, and a boiler of this kind recently placed on a steam-tug (the *Fairy Dell*) has been successfully worked at a pressure of 150 lbs. per sq. in. This, for marine purposes, is a very unusually high pressure.

*Archimedian Screw for Lifting Water.*—Mr. Wilfrid Airy has communicated a very interesting paper on this well-known contrivance for pumping purposes. He has greatly improved the screw, and made it more easy of construction by making the diaphragm which forms the spiral chamber part of a developable surface instead of part of a true screw surface. Round a solid core he winds a plane sheet of tin or other metal, and retains its inner edge in a spiral groove. The plane then takes a determinate position, not at right angles, but inclined to the central core. The whole is then placed in its usual cylindrical case. The developable screw threads are not only more easily constructed, but they make a more efficient machine than the true screw threads. Mr. Airy's experiments show that the efficiency of the screw under the most favourable circumstances may reach 85 to 88 per cent.

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## MEDICAL SCIENCE.

*Inca Skulls.*—It would appear from a letter of Herr Gratian, of Brunswick, to Chevalier von Haidinger, of Vienna, that the above subject has engrossed the attention of the former of these two savans. The following is an extract:—"With regard to my palæontological researches, I beg to inform you that they are at present in a somewhat modern direction. The exploration of beds containing fossil bones, especially of the period of the mammoth, the cave-bear, &c., as well as the search after implements of the stone period, combined with cave-studies, form now my chief occupation. I have here explored a bed which has already yielded interesting results. The acquisitions of last year include two Inca skulls from Chincha Alta, which are in a condition quite as described by Morton, and are especially distinguished by the vertical descent of the occipital bone. These skulls were, besides other curiosities, presented to me by the commander of the North German frigate *Neptune*, who obtained them at the Huacas. There is a peculiar interest attached to them in as far as these skulls were brought to the surface in consequence of the earthquake on the Peruvian coast, which happened in the month of August, 1868."—*Mittheilungen der Anthropologischen Gesellschaft in Wien.*

*Cancer of the Lymphatics.*—Dr. Whitall, who has devoted some attention to this subject, states that in a case which he lately examined after death, most of the lymphatic glands, the left breast, and the surrounding indurated tissue, contained an abundance of fibrous tissue, in which were embedded free nuclei and nucleated cells, of various shapes and sizes. In some of the glands, and in a portion of the pancreas, the cells predominated over the fibrous stroma. The central portion of the various growths was in an advanced state of fatty degeneration; in some places scarcely anything but fat was discovered; in others the cancer-cells were more or less filled with oil-globules. Portions of the pectoral muscles were reduced to mere

fibres infiltrated with cancer-cells, but contained little fat. No suspicious elements were found in the stomach or in the nodule of the spleen. The liver-cells were large, many of them hyaline and without a nucleus, others nearly normal. A good deal of free oil, but not an abnormal amount, in the cells; no excess of fibrous tissue. Some of the tubes of the kidney were infracted with granular and fatty epithelium; many of them healthy. No abnormality noticed in the tufts. There was a considerable excess of fibrous tissue.—*New York Medical Journal.*

*Cold Drinks and their Influence over Blood-Pressure.*—It is asserted by the "Journal of Anatomy" (November), that Hermann and Ganz ("Pflüger's Archives," 1870, p. 8) have endeavoured to ascertain what may be the reason for the widely-spread belief that cold drinks are dangerous during a heated state of the body. They injected water at a temperature of 0° C. into the stomachs of dogs. The blood-pressure always rose after an injection. This result cannot in their opinion be ascribed to absorption, because it appeared very speedily after the injection, and moreover hot water failed to produce it. The tracing obtained by the kymograph further showed that the increased pressure was not due to increased cardiac action; they therefore ascribed it to contraction of the vessels due to the cold. They suppose that the evils which are commonly ascribed to drinking cold water during a heated state of the body are due to the sudden increase of blood-pressure which the cold produces, favouring congestion of the brain and lungs. Quite in opposition to what one would have anticipated, they found the increase of pressure much less in animals previously paralysed by curare. They fancy that when an animal is not so paralysed, the cold, by increasing the frequency of the respirations and the depths of the inspirations, brings into play a compensating mechanism which keeps the pressure from rising so much as it otherwise would (?) It is satisfactory to know that they intend to investigate further.

*Action of Alcohol on the Body.*—A new medical periodical, styled "The Doctor," gives a note in its January number on the above subject. It says that Dr. Heinrich Timmerberg ("Inaug. Dissertation," Dorpat, 1869) found, as the results of his investigations and experiments on animals:—1. That alcohol constantly lowers the bodily temperature. 2. That it lessens the frequency of the heart's contractions. 3. That the blood-pressure in the carotids is lowered, indicating diminished force in the cardiac action, and that this effect was produced partly by direct action on the heart and partly through the vagus nerve. The retardation of regressive metamorphosis by means of alcohol is to be ascribed to the weakening of the heart's action, as well as to direct influence on the blood.

*A Physiological Prize at Cambridge.*—Dr. J. Gedge, who went out with Sir Samuel Baker's expedition to Africa, and whose death at Khartoum has been lately recorded, has left 1,000*l.* to the University of Cambridge, in order to found a biennial prize for physiological research.

*Dr. Norris's recent Experiments as to Blood.*—At the recent soirée of the Royal Society, among the many curiosities exhibited, the most remarkable, and what had most interest for members of the medical profession and for physiologists generally, were the experiments performed by Dr. John Norris to show the cohesion of colloidal films and spheres, by which he believes that

the passage, *en toute pièce*, of the blood-corpuscles through the capillary walls, as observed by Cohnheim and others, can be explained. Dr. Norris used for the purpose a solution of soap and metal rings of various diameters, set in handles, to hold the films, the spheres being the ordinary blown soap bubble. When such a bubble was allowed to fall upon a film, it at once assumed a flattened ovoid form, and projected equally on either side of the film, moving readily across it as the frame was inclined from one side to the other. By a dexterous application of the blowpipe Dr. Norris next took away the bubble from the opposite side of the film to that to which it had been first applied, leaving the latter unbroken. Thus the sphere had passed through a film which presented no opening of any kind without rupturing it. Solid bodies well moistened externally, such as an orange, or a mass of glass, were also passed through. A certain proportion of moisture Dr. Norris has found to be essential to the cohesion of the colloidal substances; for if the sphere and the film were kept apart for a few seconds, no such result ensued; the sphere rested on the film without cohering to it or changing its shape. Applying the knowledge acquired by these experiments to the migration of the blood-corpuscles through the capillary tissues, it may be conjectured that such a relation as regards moisture exists between the corpuscle and the wall of the vessel in the normal state as to prevent the passage of the one through the other; but if this relation be disturbed by any cause, cohesion of the opposed surfaces occurs, and the sphere (blood disc) passes through the film (wall of the capillary). The aggregation of the corpuscles into rouleaux, under certain conditions, may also perhaps be similarly accounted for.—*Vide Lancet* (March 18).

*How are the Miasmata of Marshes destroyed?*—An interesting controversy is at present going on, says "The Lancet," in Italy between Dr. Fattorini and the well-known Dr. Pantaleoni, of Rome. The latter stated broadly, at the Congress of Florence in 1869, that the most efficient manner of rendering marsh land healthy is to allow a large population to inhabit it. He gave as an example a portion of central France called Sologne, which formerly was very deadly, owing to marsh miasmata, and which now, being densely populated, has become a very healthy district. These opinions were repeated and dilated upon by the same author in the Italian journal "Lo Sperimentale" (September, October, and November, 1870). Dr. Fattorini retorts, however, that draining is the principal means of lessening the unhealthiness of such districts, and that the natural consequence of Dr. Pantaleoni's tenets would be that people should be thrust into unhealthy localities to diminish the amount of miasmata.

*Fusion of the Anthropological and Ethnological Societies.*—We are happy to say that this fusion has been at last accomplished. The title of the new body, named by Professor Huxley, is the Anthropological Institute of Great Britain and Ireland.

*Health of Baron Liebig.*—We rejoice to hear of Baron Liebig's recovery. He is now lecturing at the University of Munich with all his old energy.

*Platynemic men in Denbighshire.*—The remains of these men has been the subject of a communication of much interest by Mr. Boyd Dawkins, F.R.S., and Mr. Busk, F.R.S., to the "Journal of the Ethnological Society"

(January). It seems that in these men the tibiæ were remarkably compressed laterally, giving to transverse sections of the bone almost the form of a vertical antero-posterior section of a canine tooth, instead of the irregularly rhomboidal form by which they are usually characterised. This peculiarity was first noticed by Dr. Falconer and Mr. Busk in 1863, in human remains procured from the Genista cave at Gibraltar, and almost coincidently by Professor Broca, in tibiæ procured from the dolmen of Chamant (Oise), and subsequently in those discovered at Montmartre by M. E. Bertrand. Mr. Busk considers it in the highest degree improbable that it constitutes a race character, and still less that it can be looked upon as indicative of simian tendencies, a notion that M. Broca seems inclined to favour. The remains were found in a cave at Perthi Chwareu, near Corwen, with bones of the dog, fox, badger, pig, roe and red deer, sheep, Celtic shorthorn, horse, water-rat, hare, rabbit, and eagle, and in another at Cefn, near St. Asaph, where the tomb was remarkably divided into chambers. There appears to have been at least sixteen bodies, and Mr. Dawkins refers them to the Neolithic age.

*Influence of Quinine on Temperature.*—The "Indian Medical Gazette" for Dec. 1, 1870, contains a short paper by Assistant-Surgeon Dr. Hamilton, of the Royal Artillery, on this subject. It appears, says "The Lancet," which gives a short account, as the result of his experiments on an officer, aged forty, of spare habit of body and nervous temperament, who was the subject of ague, that quinine administered in ten- and five-grain doses had the effect of averting the paroxysms, and of reducing the temperature about 3° as determined by one of Cassella's most delicate registering instruments. It had been previously suggested by Assistant-Surgeon Hall, also of the Royal Artillery, that quinine should be administered internally and hypodermically in cases of insolation; and, if we remember aright, some cases illustrative of the apparent benefit of quinine were published by him. If it be proved that this alkaloid has this property of reducing the temperature, its effect in such cases where the blood becomes super-heated may, as Dr. Hamilton points out, be explained.

*A supposed Difference of Blood between Races.*—Dr. R. H. Bakewell, in a paper in the "Journal of Anthropology," January, makes a series of statements which we cannot at all accept. He says, for example, he found that between the blood of the flesh-eating Mussulman and the Hindoo, although coming from the same place, there was a marked distinction. The Hindoos' blood contains a much larger number of white corpuscles; the red corpuscles are smaller, less numerous, not so round in outline, the edge being sometimes almost stellate, or serrated, whilst they never, so far as his observations went, ran together like rouleaux of coin. Now it is well known that this phenomenon is described, in all books on physiology, as a characteristic of healthy human blood. The red corpuscles of the Hindoo, however, run together edge to edge, but not side to side, and thus form, under the microscope, a flat mass. This often, when the patient is weakly, or has had intermittent fever, becomes a sort of "squashy" mass. It seems as if the weight of the thin glass cover had crushed the corpuscles into one flat mass, in which the separate corpuscles could no longer be distinguished. "In noting down my observations I was obliged, for brevity's sake, to give a name to

the phenomenon of aggregating like rouleaux of coin; I therefore call it 'nummulating.' A defect or absence of this power is found in all persons whom I have examined, who have been for a long time subject to malarious fevers."

## METALLURGY, MINERALOGY, AND MINING.

*The Progress of Iron and Steel Industries.*—Mr. David Forbes, F.R.S., the foreign secretary to the Iron and Steel Institute, has just published, in the latter's journal, a very valuable report of the progress made both in England and the Continent during the past quarter. From this we find that though the Iron industries have been stopped in France by the war, yet that during the first half of the year their returns were greater than last year. Thus:—

| | Cast Iron. Tons | Wrought Iron. Tons |
|---|---|---|
| 1st half-year 1870 . . . | 714,892 | 510,528 |
| 1st " 1869 . . . | 699,749 | 497,328 |
| | 15,143 | 13,200 |

Again, the production of steel for the first half-year 1870 is thus estimated:—

| | Tons |
|---|---|
| Bessemer steel . . . . . . . . . | 25,360 |
| Martin and other steel . . . . . . | 44,219 |
| Total . . . . . . . | 69,579 |

The Report deals with the produce in the different countries of the world, such as Germany (the different States of), North America, the United States, Norway, Russia, Spain, Sweden, Upper Silesia. It contains, besides, abstracts of the more important means employed of manufacture, and is altogether a most valuable essay, which it is to be hoped the Society will continue to enable Mr. Forbes to bring out.

*Mechanical Properties of Steel possessing Phosphorus.*—M. Gruner, Professor of Metallurgy at the School of Mines at Paris, has published in the "Annales des Mines," 1870 (xvii., p. 346), a paper on the Mechanical Properties of Steel containing Phosphorus. Premising by stating that in a previous memoir on the Heaton process ("Examen du Procédé Heaton," Paris, 1869) he had sought to prove (1) when pig iron containing phosphorus, but poor in silicon, is refined with nitrate of soda, that, although the greater part of the phosphorus is eliminated, it still retains two or three thousandth parts of this substance, if the amount of nitrate employed be below 13 to 15 per cent. of the weight of the pig iron.. (2) That these two or three thousandths of phosphorus will render the product more or less brittle. (3) That the presence of the phosphorus increases up to a certain point the resistance to fracture, provided it be tested by a slow and gradually applied force. (4) That, as before shown by Dr. Wedding, steel not containing more than 0·005 of phosphorus may be easily worked cold; goes on at length into the consideration of the mechanical properties of certain samples of steel, the testing of which had been conducted by Mr. Fairbairn, whose results are given in a

comprehensive table, and, as the result of this inquiry, Professor Gruner arrives at the following conclusions: (1) that phosphorus when present in steel in the proportion of from 0·002 to 0·003 renders it rigid and elastic; increases its elastic tension and resistance to fracture, without altering its hardness; but that such steel, even if it contains but little carbon, wants "body," and is brittle, without being at the same time hard; (2) in order to show this want of "body," the tests of simple traction and transverse pressure are not sufficient; it requires testing by blows or shocks. (3) That soft Bessemer steel, produced from hematite, at Barrow, possesses less tenacity and elasticity, and is more brittle than the soft, or extra soft, Sheffield crucible steels; and (4) that steels containing phosphorus are deficient in "body," and that it is at present premature either to consider the Heaton process as a great improvement in steelmaking, or that the steel prepared by this process can be favourably compared with the usual Sheffield product.—*Quarterly Report of the Iron and Steel Industries*, 1871.

*A Novel Rolling Mill.*—In the Report above quoted from we find the following interesting account. The new mill is considered a novelty, the invention of M. Roy, being the universal rolling mill erected by him at the Savona Works. These rolls are figured and described in the September number of "Il Politecnico," "Laminatojo a cilindri universali per la produzione dei ferri rettangolari" in which machine, by means of a movable cylinder, or ring, sliding over smooth rolls, and held in position by a large screw collar, working on a thread cut on the rolls themselves, the groove in which rectangular iron is rolled may be increased or diminished in size, so that the same pair of rolls may serve for rolling various dimensions of iron, without having any grooves whatever cut in them. It would require the assistance of an illustration to explain exactly how this arrangement is constructed, notwithstanding that it is of an extremely simple nature, and well adapted for small rolling mills; although, for large establishments, it would most probably be better to adhere to the usual system of having the separate grooves cut in the rollers themselves. A short description of this system, with an accompanying plate, will also be found, in French, given by M. Lemut, in the "Revue universelle de Mines et de la Métallurgie," 1870, p. 250.

*A New Locality for Meneghinite.*—In Poggendorff's "Annalen" (No. 11, 1870), Herr A. Frenzel describes a locality in Germany where the mineral alluded to has been found by him. The mineral, on being examined, was found to possess a sp. gr. of 6·367. The chief constituents of this substance are lead, antimony, and sulphur; in 100 parts—Lead, 63·89; antimony, 18·82; sulphur, 17·29.

*The Rare Mineral Gahnite.*—Mr. G. J. Brush describes this mineral from specimens in Mine Hill Franklin Furnace, New Jersey, U.S.A. He (in the "American Journal of Science and Arts," January 1871) describes the mineralogical characters of this mineral found in a zinc mine. The mineral is crystalline; colour, blackish-green; hardness, 7·5; sp. gr., 4·89 to 4·91; infusible before blowpipe; with fluxes, reacts for iron and manganese, and with soda on charcoal gives a zinc coating. The composition of this mineral in 100 parts, is—alumina, 49·78; ferric oxide, 8·58; zinc oxide, 39·62; manganous oxide, 1·13; magnesia, 0·13; silica, 0·57. This variety of gahnite

(so named after the celebrated Swedish mineralogist, Gahn) shows a larger percentage of zinc than any specimen of this mineral heretofore analysed; it is associated with black mica, apatite, calcite, and a brownish variety of chrysolite, which, on partial analysis, was proved to be a mono-silicate of iron, manganese, and zinc.

*The Zircons of Mudgee, New South Wales.*—Professor S. H. Church, M.A., gives an account of these in a recent number of the "Chemical News." He says he lately obtained a few rounded pebbles, each weighing about 2 grammes, for examination: they came from Mudgee, New South Wales. Although the specimens did not present the usual lustre of worn surfaces of zircon pebbles, yet their obviously high density, and the traces they retained of their pyramidal form, nearly sufficed to identify them with the zircon; this idea was amply confirmed by the results of experiment. The Mudgee zircons present the exact tint known as hyacinthine; indeed the true hyacinth or jacinth, about which such constant mistakes are made by jewellers, lapidaries, gem-collectors, and even mineralogists, is now an attainable luxury. The engraved gems and the cut stones commonly called jacinths (even by Dana—*vide* his "Mineralogy," fifth ed., p. 275), are invariably, so far as his experience goes, nothing but the hyacinthine garnet, a comparatively common stone possessed of far less interesting properties than the true hyacinth. The Mudgee zircons are rather dark; the colour is distributed somewhat irregularly in and upon many of the specimens. When cut, facetted, and polished, this Australian zircon, if not too deep in colour and too large in size, presents a rich soft red colour tinctured with orange-brown. He was fortunate enough to secure one pale-coloured specimen, which, owing to its having been judiciously cut, has turned out a stone of surpassing brilliancy and beauty. The density of one of the Mudgee stones was 4·704. After heating, it was found to have become quite colourless, although its density remained virtually unchanged, namely, 4·699. In these and most other particulars the Mudgee specimens resemble those of similar colour from Expailly, in Auvergne.

*The Influence of Cold on Iron and Steel Railway Wheels.*—This subject has been very largely taken up this year at Manchester, and the "Proceedings of the Literary and Philosophic Society" have been filled with papers which are some of them exact contradictions of others. The following observations were made by Mr. Peter Spence at the meeting held March 10, 1871. After detailing some experiments, he says, his assistant then prepared a refrigerating mixture which stood at zero, and the bars were immersed for some time in this, and they prepared for the breaking trials to be made as quickly as could be, consistently with accuracy; and to secure the low temperature, each bar on being placed in the machine had its surface at top covered with the freezing mixture. The bars at zero broke with more regularity than at 60°; but, instead of the results confirming the general impression as to cold rendering iron more brittle, they are calculated to substantiate an exactly opposite idea, namely, that reduction of temperature, *cæteris paribus*, increases the strength of cast-iron. The only doubtful experiment of the whole twelve is the first, and as it stands much the highest, the probability is that it should be lower; yet, even taking it as it stands, the average of the six experiments at 60° F., gives 4 cwts. 4 lbs. as

the breaking weight of the bar at that temperature, while the average of the six experiments at zero gives 4 cwts. 20 lbs. as the breaking weight of the bar at zero, being an increase of strength from the reduction of temperature equal to 3·5 per cent. Sir W. Fairbairn, in a paper read another evening, attributes the breaking of the wheel in railway carriages to irregularity of the action of the wheel, caused by alteration in position of the tire.

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## MICROSCOPY.

*Nobert's 19th Band and its Observers.*—The "Monthly Microscopical Journal" for March contains an important paper by Mr. Charles Stodder. In it he opposes the statements of Col. Woodward, that he had not seen the 19th band on the instruments he asserted he had seen it with. He seems very fully to demonstrate his position, but probably there will be an answer by Col. Woodward in next month's number.

*How to Mount Objects* is very well treated of, and discussed by Mr. D. E. Goddard, in the "Journal of the Quekett Club" for January. In the same number Dr. Bastian's views are opposed by Mr. Lowne, and an ingenious neutral-tint selenite stage is described by Mr. W. Ackland.

*The Largest Angle of an Immersion Lens.*—Mr. Wenham, writing in the "Monthly Microscopical Journal" for January, says that the same optical law that limits the aperture of any object-glass to near 82° in a balsam-mounted object also determines the angle *in the lens* at which the rays diverge *after* being refracted from the plane surface of the front. *This can never exceed* 82° in a dry objective; nor can it be greater on the immersion system, where an interchange of front adapts it to both conditions, as the very correction which necessitates the form of the back lenses and their diameters will not transmit a greater pencil; and therefore, if the front is immersed in balsam for the purpose of viewing an object placed therein, this angle of 82° or less, as the case may be, instead of converging at 170° as from the dry lens, is continued right to the object, supposing the refractive index of the front and balsam to be the same, which they are nearly.

*Phycocyan.*—Mr. T. Charles White has described to the Royal Microscopical Society how this substance came to be developed in a bottle of fluid collected in the round-water at Kensington Gardens, and left aside. The following are the author's words:—"Not thinking this worth exhibiting to my friends, I screwed down the top of my York bottle, and stood it on the window-ledge inside my room, and it was there forgotten for about a fortnight, when, by chance looking up at it, I saw that the green flocculent matter had descended to the dead level of some yellowish-coloured mud, but for about a quarter of an inch above it was a layer of the deepest richest indigo blue I had ever seen. Taking it down carefully, so that no disturbance of it should take place, I looked at it by reflected light, when it was a rich crimson lake. I then remembered the dichroic fluids I had seen here, and made a careful examination." This examination, however, does not throw more light on the subject.

*Browning's Microscope Lamp,* which has been made since our last issue, is one of the nicest instruments we have seen. Not only is it complete optically, but it is admirably handy, packing into a small case, just six inches

high by three inches wide. It is an instrument which no one who works at the microscope should be without. The following is the account of it given by the maker, and we can fully bear out its accuracy: The metallic chimney being telescopic occupies a very small compass; the condenser fits into the cell in front, which is also provided with plain and tinted glass for correcting the colour of the flame. The reservoir is of brass, and will contain sufficient petroline for six hours' consumption. The entire lamp fitting into the case from the top, escape of the oil is prevented. In trimming the lamp care should be taken that the wick is perfectly dry, and the petroline of good quality; also that none of the oil gets upon the metallic chimney or reservoir, or a bad smell will be given off until the oil is burnt away. In using the lamp it will be found convenient to slightly incline it, so as to bring the broad surface of the flame more parallel with the surface of the mirror of the microscope. When it is necessary to re-line the chimney, screw off the sliding portion, wash out the old lining, and re-coat it with superfine plaster of Paris. When dry it will be found ready for use—a few minutes will be found sufficient to do this.

*The American Journal of Microscopy* is, so far as we have seen, a most inferior Journal of general natural history. It is many miles behind "Science Gossip" in point of matter. However, it may improve.

*The Aëroconiscope.*—This name has been applied, we imagine, by Dr. Maddox. Through this instrument he collected the various germs which have been floating in the air for months. Some of the fruits of his research have been figured in two plates in the "Monthly Microscopical Journal" for February, but as yet a great number of the specimens obtained by him with the above instrument and figured in the Journal, are unnamed.

## PHOTOGRAPHY.

*New Lens.*—A new lens, of which great things are reported, has recently been brought out by Ross. Including the full pictorial angle of view, it possesses the further advantages of being quite free from distortion, working with great rapidity, and having so much sharpness that a figure which was taken in the foreground of a landscape has been magnified up to a large size; the magnified photograph being quite sharp enough for pictorial effect. With a lens of this kind the tourist photographer has a great power, viz. that of being able to select and magnify any desirable object in the negative taken by him during his journeyings.

*Pocket Cameras.*—The advisability of using very small cameras for landscapes has been much discussed in photographic circles during the past two or three months. It is well known that from a very small negative an enlarged print may be obtained, the degree of enlargement depending upon two things—the nature of the deposit that forms the image in respect of its fineness and delicacy of gradation, and the sharpness of the picture. Now, in consequence of an inexorable law in optics, it is impossible to obtain definition of the highest class over more than a very small space in the centre when a *flat* plate is used on which to take the photograph, for in flattening the field of a lens intended to work with a large aperture an amount of astigmation is introduced which is quite fatal to sharpness. Now,

astigmation can only be reduced within the necessary bounds by allowing the pencils transmitted obliquely through the lens to be brought to a focus nearer to it than can be done on a flat plate; hence the attention of opticians is now being directed to the production of portrait-lenses for working on *curved* instead of flat-glass plates, the advantage of this arrangement being that a landscapist can thus obtain a small but exceedingly sharp picture with an instantaneous exposure, and that an enlarged print of great dimensions may be obtained from a very small negative of this class.

*Reproducing faded Prints.*—At a recent meeting of the Photographic Society a successful method of reproducing faded photographs was communicated by Mr. Pritchard. The faded print, having been first of all removed from its cardboard mount, is saturated with wax, so to render it more transparent. It is now superposed on a plate of glass coated with collodio-chloride of silver; this coating is rendered more sensitive by being fumed with ammonia previous to exposure to the light. After being printed, the image on the glass plate is strengthened by being washed over with an intensifier composed of

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| Gallic acid . . . | 75 grains |
| Glacial acetic acid . . | 2 drachms |
| Acetate of lead . . . | 50 grains |
| Distilled water . . . | 20 ounces. |

A little silver solution is added to this mixture where great intensity is required. By using this plate as a negative, prints may be obtained which are greatly superior to those from which the negative was produced.

*The late Rev. J. B. Reade, F.R.S.*—As one of the early photographic experimentalists, this gentleman was much esteemed. His death, which took place on December 12, removes from amongst us the last but one of the "fathers of photography." He was the first to employ gallic acid as a sensitiser in photography, and by its agency in connection with silvered paper he, in 1837, obtained enlarged views of minute objects by the agency of the solar microscope. He was also the first to publish the use of hyposulphite of soda as an agent for fixing photographs.

*Artificial Light for Photographic Enlargements.*—Dr. Monckhoven has published the method by which he prepares magnesian blocks for producing an intensely actinic light by the oxy-hydrogen burner. His artificial magnesia is thus made:—Mix four pounds of English caustic, or calcined, magnesia with two pounds of carbonate of magnesia, add one pint of water, and knead them into a tough uniform dough. This dough is put into an appropriate box and pressed into a compact mass; after which cut it in equal parallelopipedic pieces, and dry them for a few days in an oven, and afterwards make them red-hot for about half an hour.

*Preserving the Purity of Sensitive Paper.*—The following method is recommended by an American writer, by which he says he can retain his sensitive paper for several days in good condition. To a bath composed of one ounce of silver to sixteen ounces of distilled water, he adds a few drops of liquor ammoniæ followed by an ounce of nitrate of ammonia. On this bath the paper is floated for one minute; it is then drawn over the sharp edge of the dish for the purpose of removing all the surface solution, and is laid, face downward, on a quire of bibulous paper, covered by a couple of sheets of the same paper. Friction by the hand causes all the superfluous silver to

be removed from the sensitised sheet, which is then hung up to dry, and the same operation is repeated with others, the lower sheets of bibulous paper not requiring to be renewed until many sheets of sensitive paper have been prepared.

*New Preservative for Dry Plates.*—Mr. Carey Lea has found that the well-known developing agent, pyrogallic acid, also acts the part of a preservative for dry collodion plates, giving a greater degree of sensitiveness than most of the preservatives now in use. After many experiments, he recommends the following as being the best way to use it:—A stock solution is prepared containing one ounce of pyrogallic acid to eight ounces of alcohol; and of this, which contains rather under sixty grains to the ounce, a half fluid drachm is added to eight ounces of water in which has previously been dissolved eighty grains of gum arabic and eighty grains of white sugar. It is better that the pyrogallic acid be added to the other solution just before using. This, in the estimation of the inventor, forms the best and most convenient preservative for dry plates hitherto introduced.

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## PHYSICS.

*The Bessemer Flame seen with Coloured Glasses and the Spectroscope.*—The "Chemical News" of January 20 contains a paper on these subjects by Mr. J. Spear Parker. With regard to the flame he says, the combinations of glasses used by Mr. Rowan and Professor Silliman for observing this flame are somewhat similar in their effect, the two light yellow in the one case being nearly equivalent to one dark yellow in the other. The combination he used was one cobalt-blue glass of rather light shade, and one amber-coloured; too deep a blue he thinks should be avoided, otherwise the flame merely shows varying shades of crimson. The appearances observed were similar to those recorded by Professor Silliman. On first turning on the blast, the flame appeared of a reddish-orange; after the lapse of a few minutes, this gradually changed to crimson, first in flashes, afterwards continuously; the sodium line appearing in the spectroscope at exactly the same time, and corresponding also to the flashes; after a few more minutes had passed, the flame altered to yellow, and, at that time, the characteristic Bessemer spectrum appeared, flickering at first, like the sodium line; when it became steady, the flame, when viewed through the coloured medium, appeared to consist of a sheath of a light yellow colour, the inner cone being rose-red, while the upper part of the flame was edged with crimson; this appearance was maintained throughout the rest of the decarbonisation, only varied by occasional flashes of a deeper red, or of a greenish hue. When the termination of the reaction was attained, the flame, after a few premonitory flashes, changed again to the crimson colour. The appearance of a crimson ring round the mouth of the converter he is inclined to attribute to irradiation.

*Indian Pendulum Experiment.*—Col. Walker, Superintendent of the Trigonometrical Society of India, has written to the Royal Society to say, that he proposes that Captain Basevi should proceed from Karachi to England, taking observations *en route* at Aden and in Egypt, and bringing his operations to a close by a series of observations at the Greenwich Observatory, if the Astronomer Royal has no objection. He mentions the Green-

wich rather than the Kew Observatory because the true time can be obtained there from the astronomical clocks, whereas at Kew it can only be obtained by observation; and if (as is probable) Captain Basevi arrives in the winter, pendulum-observations taken at Kew would be greatly delayed, as happened when the operations were commenced at Kew. Moreover, Greenwich appears to have been employed as a reference station for pendulum-observations more frequently than Kew.

*Electro-motive Force.*—We desire to direct the attention of those who are interested in this subject to a series of papers, one of which is published in the "Chemical News," January 6, written by the Rev. W. Highton, M.A. The author's observations are too long for abstract, but they certainly tend to prove that there is no ordinary mechanical law applicable to electrical force. In other words, he goes in against the doctrine that only a definite amount of work can be obtained from a definite amount of chemical change, and he seems to us to prove his case clearly.

*The Fusibility of Platinum-wire by the Blowpipe.*—Mr. E. J. Chapman, of Toronto, Canada, does not think much of Dr. Skey's supposed discovery of the fusibility of platinum-wire recorded in our last. He says: "If Dr. Skey will look in Plattner's 'Probirkunst,' p. 16 of last edition (p. 14 of third edition), he will find the following statement: 'When it is desired to test whether it is possible to produce a sufficiently strong oxidising flame, it is only required to try to fuse the end of a platinum-wire of a thickness of 0·1 mm. to a globule. The wire is bent for this purpose at a right angle, and the shorter bend is so held in the outer flame that the axis of the wire exactly coincides with the axis of the blowpipe-flame, care being at the same time taken that neither wire nor flame vibrate. With strong and really good flame a globule of molten platinum is very soon formed, and this globule will be the larger according to the greater strength of the flame.' A similar method of showing the fusibility of platinum by the blowpipe is given by Dr. H. O. Lenz, in his 'Löthrohrschule' (Gotha, 1848), and Bruno Kerl, in his handy little 'Leitfaden,' refers also to the fusibility of fine platinum-wire by the blowpipe-flame. Other published statements of this well-known fact might likewise be quoted."—*Chemical News* (January).

*Duration of Lightning Flashes.*—Mr. O. N. Rood has been carrying out researches on this subject, and the result of his experiments is that the duration of flashes of lightning, as observed by him, and measured by means fully described in this memoir, during a violent thunderstorm in August last, amounts, in round numbers, to about 1-500th of a second, the average length of the streak being 9°.—*American Journal of Science and Art.*

*Water Supply to Towns.*—Herr E. Grahn has published a paper in the "Journal für Gasbeleuchtung" (December), which is a portion of a lecture given by him before a meeting of water-works engineers, held at Hamburg last May, and contains, apart from matters strictly relating to the engineering part of water supply, a very excellent account of the origin of water in nature, of its various functions in the three kingdoms of nature, and on the requisite qualities of water intended for domestic use.—See also *Chemical News.*

*The Constitution of the Sun.*—This paragraph should be in our Astronomical summary, but as it is not there we insert it here. It relates to M. Zöllner's recent paper in "Poggendorff's Annalen" (No. 11, 1870). This essay, says the abstract in the "Chemical News," illustrated by a series of

very beautifully executed chromo-lithographs, is, as regards its contents, entirely of an algebraical character. The author deduces, however, from his investigations the following condensed results: (1) The absence of the lines of some element in the spectrum of a self-luminous body (star) does not prove the absence of this substance. (2) The layer in which the inversion of the spectrum takes place differs for every body, and that layer is placed the nearer to the centre of the star according to the greater vapour density and emissive power of the substance which yields the spectrum. (3) This layer is, *cæteris paribus*, nearer to the centre of different stars according as the intensity of the gravitation thereof is greater. (4) The distances of the layers of conversion of the simple bodies (emitting spectra) from each other, as well as from the centre of the stars, increase with an increase of temperature. (5) The spectra of different stars are, *cæteris paribus*, the richer in spectrum lines according to the lower temperature and the greater mass of the stars. (6) The great difference observed in the intensity of the dark lines of the solar spectrum and that of other fixed stars does not simply depend upon the difference of the power of absorption, but also upon the difference of depth wherein the inversion of the respective spectra takes place.

*A Topographical Survey of the Hawaian Islands* has been ordered by the Legislature of the Islands, says Silliman's "American Journal" (January), and an appropriation of $5,000 made for procuring instruments and meeting the expenses of the first year. Professor W. D. Alexander of Oahu has been appointed Surveyor-General, and is making arrangements for commencing the work. He proposes to measure a base line on the sandy isthmus between East and West Maui, which is six or seven miles wide, and to carry forward the survey as nearly as possible after the methods of the U.S. Coast Survey. Geological and botanical collections and observations will be made in connection with the survey.

*The Spectrum of the Aurora Borealis.*—This has been well investigated by Mr. John Browning, who gives his results in the "Monthly Notices of the Royal Astronomical Society" (November 11). During the display of the Aurora Borealis which occurred on the evenings of October 24 and 25, he confined his attention to observing the spectra of the light, taking it in different parts of the sky. When the spectroscope was directed to the more luminous portions, which were generally of a silvery white, the spectrum appeared to consist of only one line. He could not succeed in verifying the position of this line; but it appeared to be situated between D and E in the spectrum. When observing the light of the red portions of the sky, a faint red line became visible. He had no means of verifying the position of these lines with any degree of exactitude; but he was able to throw into the field of view a faint continuous spectrum from a distant light, and also the bright yellow sodium-line produced by a spirit-lamp. The colour of the green line was very peculiar; had he not been able to observe it by comparison, he could not have formed any idea of its position. It was an exceedingly light silvery green, or greenish-grey, and often seemed to flicker. Besides the two lines particularly described, he occasionally suspected others, one in the red and one in the blue; but he could not be at all sure of this. The colour of the light of the aurora seen over the greater portion of the heavens resembled exactly that of the discharge of electricity from an induction-coil through a vacuum formed from atmospheric air.

## ZOOLOGY AND COMPARATIVE ANATOMY.

*The Crustacea of the Gulf Stream.*—Those of the Gulf Stream and of the Straits of Florida have been reported on by Pourtales. The Brachyura have fallen to the hands of Dr. W. Stimpson, who describes them in the "Bulletin of the Museum of Comparative Zoology," vol. ii. number 2. In this important paper Dr. Stimpson gives a full list of the Brachyura collected by the Coast Survey dredging expeditions of 1867-8-9. Eighty-one species, representing forty-seven genera, are mentioned, and fifty-two of the species and nineteen of the genera are described as new. More than half the species belong to the *Maioidea*, while the *Ocypodoidea* are represented by only two species, both of them belonging to the *Carcinoplacidæ*. Only a small proportion of the species are from great depths, and the number of new forms seems largely due to the thorough exploration of the shallower waters. But fifteen species are recorded as coming from below 100 fathoms, and of these eleven are Maioids, and the other four are Cancroids—a *Pilumnus*, two species of *Bathynectes* (a new genus allied to *Portunus*), and a species of *Acheloüs*. The greatest depth at which any of the species were found is 150 fathoms; and it is quite remarkable that the only species from that depth were *Portunidæ*—one of the species of *Bathynectes* and the *Acheloüs*.

*The Life-History of Monads.*—One of the ablest and longest papers we have seen on this subject and in English, is by Dr. Henry Fripp, in the "Proceedings of the Bristol Naturalists' Society." It is very long, and refers to a plate, but there is none accompanying the paper. It is to be hoped that it will be properly published before long, with the plate accompanying it.

*The Representation of the Melolontha vulgaris in Canada.*—Mr. Ritchie, writing in a late number of the "Canadian Naturalist," says that the *Melolontha vulgaris* of Europe is represented in Canada by *Lachnosterna fusca*, commonly called the May bug. In reference to the appearance of this creature, we may state that it occurs in immense numbers every three years; at least, such is the experience since 1855. The years 1858, 1861, 1864, and 1867 are those when this insect appeared in greatest numbers. It must not be inferred from the above statement that no examples of these insects occurred in the intervening years, for it is always a common species in Canada. But there are years when certain species prevail in such numbers as to be noticed by everybody. One reason why the cockchafer should be tri-yearly may be owing to the circumstance that it remains in the larva state for three years. Here, then, an opportunity occurs for testing some of the alleged practical uses to which these insects may be put.

*Crustacea of the genus Libinia.*—In the Philadelphia Academy of Sciences ("Proceedings," Sept.) Mr. T. Hall Streets states, in respect of the above, that much uncertainty has existed with regard to the identity of certain species belonging to the genus Libinia. *Libinia dubia*, ever since it was first established by Milne Edwards, has been regarded as a doubtful species. In the description of it by Edwards, he states that it resembles *L. canaliculata* very much, and that it is not improbable that it is the young of that species. Naturalists in this branch of science down to the present time appear to have accepted this statement as the truth. De Kay, in his

"Natural History of New York," states that the "younger individuals, one to four inches in length, are more pyriform in shape, are entirely covered with a dense, downy hair, and the spines are not so prominent as in the adult. In this state he supposes it to be the *L. dubia* of Edwards. Gibbes, in an article in the "Proceedings of the American Association for 1850," regards the two species as distinct, but says that no absolute characters can be indicated by which they may be separated. He does not know how to account for this prevailing ignorance, as the characters existing, separating the two species, are so plain. He then goes on to give the special characters, which we must omit.

*Are the Brachiopods Annelids?*—This question is asked and answered by Mr. E. S. Morse in a late number of the "American Naturalist." He replies at great length to Mr. H. Dall, who takes a different view. The paper is too long and too vaguely written for an abstract.

*New American Fishes.*—Professor E. D. Cope says that a number of interesting additions to the ichthyological fauna of the United States having been sent to the Museum of the Academy of Natural Sciences by his fellow-member Samuel Powell, he places them on record for the convenience of ichthyologists. Several of the species, it will be observed, were new to science at the time they were received; some of them have been described by Professor Gill. Most of these are of West Indian affinity, some being simply well-known species of that region, which have wandered, as has been suggested by Gill, along the Gulf Stream, and turned aside on the southern coast of the New England States.

*A New Position for the Nematoid Worms.*—Mr. Lowne, who recently read a very valuable paper before the Royal Microscopical Society on the above subject, concludes that it is apparent that the Nematoid worms stand in a clearly intermediate position between the *Echinodermata* and *Annelida*. The water vascular system with its vesicles reminds one strongly of an Echinoderm; the pharynx, the pharyngeal teeth, and segmented integument are clearly those of an Annelid, whilst the nervous system is more nearly like that of the earthworm than that of an Echinoderm. In Gordius he thinks there can be no doubt of this, where there is but a single ventral cord. He cannot agree with Dr. Bastian's view that the Nematoid worms are more nearly allied to Echinoderms than to Scolecida, although he must think much credit is due to him for having first pointed out strong affinities with the *Echinodermata*—not stronger affinities, however, than those known to exist between Nemertids and Echinoderms.

*Insect Scales.*—Mr. S. J. McIntyre, who may be said to have given the greatest amount of attention to the subject in this country, lays down the following conclusions in a paper published in the "Monthly Microscopical Journal" for January. 1. That the principal structural feature in insect scales is corrugated membranes—a plan insuring the maximum amount of strength and elasticity with the minimum of weight. 2. That there are a few scales having one surface hackled. 3. That the ornamental requirements of scales are fulfilled either by iridescence or the possession of pigment granules in or upon the membranes. 4. That the beaded appearances seen in scales are due to the following causes, either singly or collectively:—*a.* Corrugations taking the form of hemispherical embossings; *b.* Pigments; *c.* Shadows of projections,

or folds in the membranes, either within or beyond the focus of the object-glass.

*Slaughter of Penguins and Seals.*—The "Journal of Applied Science" (Nov.) gives an account of this, as conducted in the Falkland Islands. During the whole of August and beginning of September large and countless flocks of penguins come from all directions to the Falkland Islands, and where they alight the ground is literally covered with them. This periodical migration is for purposes of reproduction. The people who make a business of killing these birds for their oil, proceed about this time in schooners capable of weathering the storms that are so common at this season. Besides a small crew, these schooners have on board a "copotar," with a gang of from twelve to fifteen men. Their only arm is a short stick. On the island to which they repair they find a rough kind of furnace that has been used the previous year, and which seems to heat one or more iron boilers, each of which is capable of holding as much as 250 gallons of oil. These islands are leased from the Colonial Government for five years at a small rent, and every exporting house has several rookeries, which are respected by the rest. The penguin-hunters are generally at their post before the arrival of their intended victims, and when these arrive and drop on the ground by millions, the men go among them and commit great havoc upon the tired birds, heaped together, whose wings are intended more as helps to swim than to fly. After the lapse of five or six hours of incessant slaughter, the "copotar" and his men generally have got enough of birds for one night's boiling. Each man immediately picks up a certain number of the dead birds, and begins to skin them. This operation is done by making a cut in the belly, and, with a peculiar knack, the whole skin, with feathers and all, comes off the bird at one pull. The account goes on further, but space will not permit us a longer abstract.

*Abdominal Antennæ of Insects (Sense Organs).*—Mr. A. S. Packard, jun., writes on this subject in the "American Naturalist" for December. After referring to Dr. Anton Dohrn's paper in the "Journal of the Entomological Society of Stettin," 1869, he claims himself to have noticed the structures as early as 1866. He says he has been able to detect sense organs (probably endowed with the sense of smell) in the short, stout-jointed, anal stylets of the cockroach (*Periplaneta Americana*), beautifully mounted by Mr. E. Bicknell. He has recently, after reading Dr. Dohrn's note, observed the sense organs and counted about ninety minute orifices on each stylet, which are probably smelling or auditory organs, such as are described by Hicks. Mr. Bicknell has counted more carefully than he did the exact number of these pits, and made out ninety-five on one stylet and one hundred and two on the other, adding "there were none on the under side of their appendages that he could see." They were much larger and much more numerous than similar orifices in the antennæ of the same insect, and were situated in single rows on the upper side of each joint of the stylets. During the breeding season a peculiar odour is perhaps emitted by the female, as in vertebrate animals, and it is probable that these caudal appendages are endowed with the sense of smell, rather than of hearing, that the male may smell its way to its partner. This is an argument that the broadly pectinated antennæ of many moths are endowed rather with the sense of

smelling than of hearing, to enable the males to smell out the females. He has observed the same organs in the lamella of the antennæ of the carrion beetles, which undoubtedly depend more on the sense of smell than that of touch or hearing to find stinking carcasses in which to place their eggs.

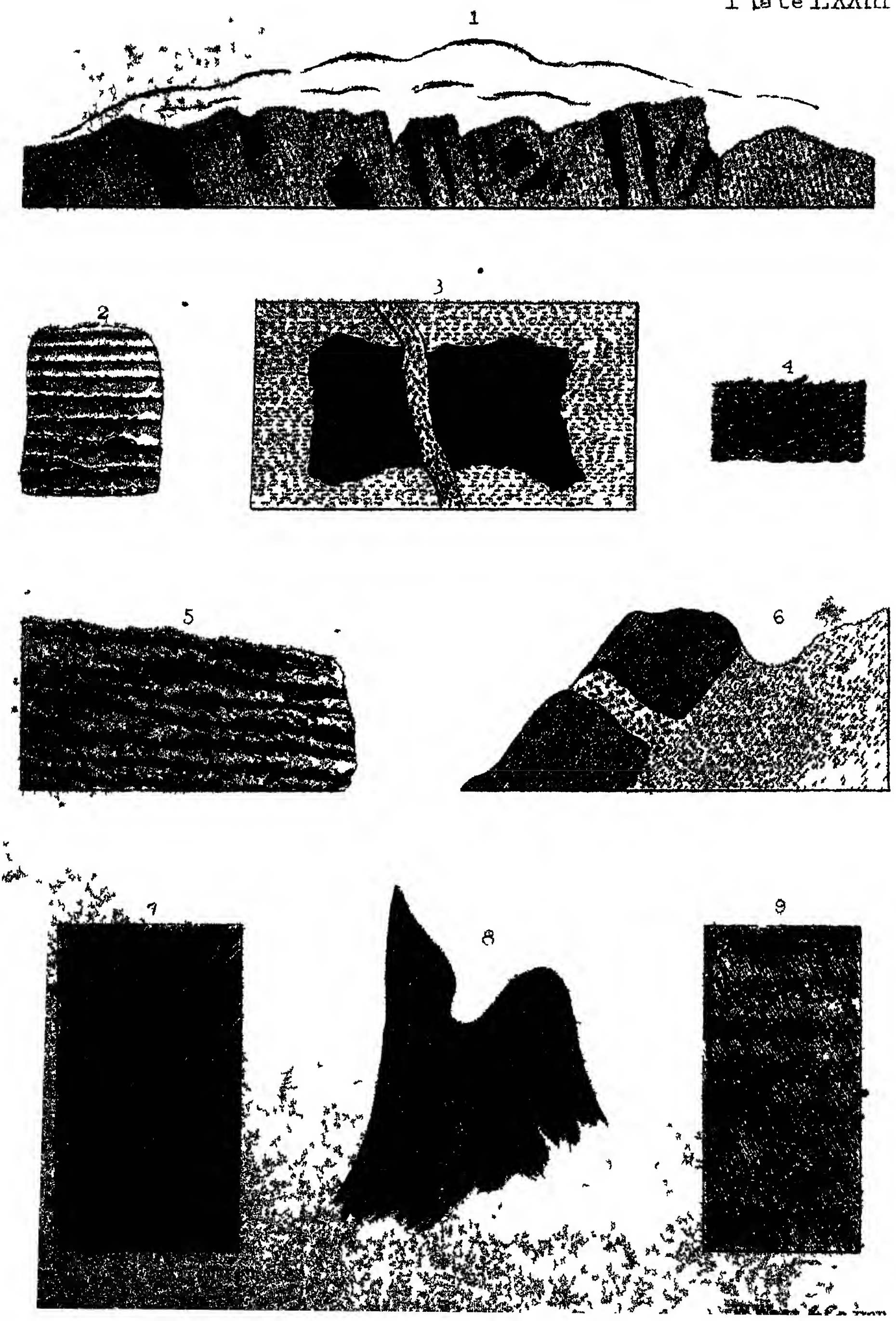
*Professor Thomson's Holtenia.*—In the "Philosophical Transactions" (Part II., 1869) appears a paper by Professor W. Thomson "on *Holtenia*, a genus of vitreous sponges," accompanied with beautiful illustrations. The genus, however, appears to Professor Leidy, who writes in the "Proceedings of the Philadelphia Academy" for November, to be synonymous with *Pheronema* (Pr. A. N. S., 1868, Biolog. and Micros. Dep. 9). A comparison of the figures of *Holtenia Carpenteri* with those of *Pheronema Annæ* ("American Naturalist," 1870, 21, 22) leads him to suspect that the two are probably the same.

*A New American Locality for Cordylophora.*—In the Academy of Sciences of Philadelphia, at a recent meeting, Professor Leidy stated that, in a recent visit to the Schuylkill river at Fairmount, to seek for specimens of *Urnatella*, though he had been unsuccessful in obtaining living ones within reach from the shore, he had found, in the same positions occupied by the former, an abundance of *Cordylophora*. This is the first time that he had noticed this interesting compound hydroid polyp in the vicinity of Philadelphia, and he was surprised that until now it had escaped his notice. *Cordylophora* was first detected by him in America at Newport, R.I. He had not been able to satisfy himself that it was a different species from the European *Cordylophora lacustris*, first described by Professor Allman of Edinburgh. It appears, however, to be much smaller. Professor Allman represents the *C. lacustris* several inches in length, with the polyps a line in length. The American is not more than half the size. As a variety it might be named *Cordylophora Americana*.

*Pterodina Valvata.*—Dr. Hudson says that the most striking peculiarity of this new species, which he figures in the "Microscopical Journal," is the presence of the large transverse muscles for folding the lorica. The lorica is oval and nearly plane, except on its under-surface, along its major axis; where it carries a sub-conical case, in which lie the greater part of the softer portions of the rotifer. The base of the cone is the opening from which the rotatory head is protruded, and the lorica is here slit to give free play to the head, while the muscles close the flaps of the slit when the head is drawn within the lorica. At a distance from the head of about two-thirds of the length of the lorica, there is a circular opening through which the false foot is protruded and withdrawn. The water vascular system with three tags on each side can be plainly seen; but there is no contractile vesicle. There are, however, two objects which appear to be expansions of the canals, and possibly answer the purpose of the contracticle vesicle: he cannot say, however, that he ever saw them contract.

*The Chair of Natural History* in the University of Edinburgh, the duties of which were so long and ably discharged by Professor Allman, has been given to Professor Wyville Thomson, F.R.S., until lately Professor of Natural History at Queen's College, Belfast. This of course creates a vacancy, the applicants for which are, we understand, numerous, but we have not as yet heard of any one being selected.

Plate LXXIII



# THE STRUCTURE OF ROCK MASSES (FOLIATION AND STRIATION).

By DAVID FORBES, F.R.S.

[PLATE LXXIII.]

IN a previous communication on this subject * the three structures, stratification, joints, and cleavage, were described; it now remains to take into consideration the two other systems of parallel structure of common occurrence in the metamorphic and eruptive rocks, viz. foliation and striation.

*Foliation.* The terms foliated rocks and foliation appear to have been introduced into geological nomenclature some twenty-five or more years ago, by Darwin, and since then they have met with very general acceptation by geologists both abroad as well as at home; by the term foliation is signified, such parallel structure as makes its appearance in rock masses owing to the arrangement of certain crystallised minerals in more or less parallel lines, along which their crystals lie on their flat sides or lengthways, i.e. having their longer axes in the direction of, and not against, the grain of the rock.

All foliated rocks come under the definition of metamorphic rocks, that is, rocks which subsequent to their *consolidation* have undergone a change in the molecular arrangement of their original component mineral particles, which change in many, if not most instances, has been at the same time accompanied by a re-arrangement of their chemical elements also.

The subject of foliation is one of the most intricate problems in geology, and opinions are much divided as to the signification and origin of these lines of parallel structure, so that, in the present communication, the views propounded must not be regarded as representing any universally accepted doctrine,

* Popular Science Review for April 1870.

but rather as the conclusions arrived at by the author, after a prolonged study of this subject in the field and in the laboratory.

Although, as before mentioned, all foliated rocks are metamorphic rocks, these may have originally been either of sedimentary or eruptive nature; for a long time it was supposed that foliated structure was alone characteristic of the so-called crystalline schists, until further inquiry into the subject showed that, besides being common in the crystalline or so-called primitive limestones, it was far from being of rare occurrence in many of the plutonic and volcanic eruptive rocks, abundant examples being met with of foliated granites, syenites, gabbros, trachytes, lavas, &c., in which a distinctly recognisable parallel structure is developed by the manner in which one or more of the crystalline mineral components are disposed in the mass of the rock.

The parallelism of foliation is not, as in the case of the other structure already described (stratification, joints, and cleavage), brought about by the formation of divisional planes due to the effects of purely mechanical forces, but, on the contrary, is invariably determined by the presence of crystallised minerals, usually in alternating layers, very different from one another both mineralogically and chemically, and which, owing to the peculiar nature (habit, or behaviour, as it has been called by mineralogists) of the minerals themselves, and to the pressure to which they have been subjected, when in the process of formation or crystallisation, by the weight of the superincumbent mass above them, most commonly assume the form of crystallised foliæ, or of crystals developed mainly in the direction of one of their axes only. From this it will be seen that it is very easy to discriminate between foliation and all other parallel structures likely to be encountered in rock masses. If at times (as in the coal formation, for example) we find minor sedimentary beds, made up almost entirely of plates of mica, or sandstones possessing a fissile or laminated structure from numerous scales of mica which may be arranged in more or less parallel lines, a closer examination of the rock, and more particularly of the mica in it—using the microscope if necessary—will at once show, that it has been deposited as a sediment from water, as the particles will be found waterworn and abraded, and to present an appearance totally different from that of the foliæ, crystallised *in situ*, which are met with in the crystalline schists or other true foliated rocks.

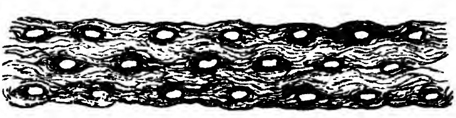
The simplest form of foliated rocks which occur in nature are those beds of crystalline schist, solely composed of one mineral, such as many of the mica, chlorite, talc, or hornblende schists; in these, as shown in Pl. LXXIII., fig. 4, the rock is seen to be a mere aggregation of imperfectly developed crystals

of the mineral mostly lying, or, as it were, drawn out in one direction; in fact, the metamorphism in this case appears to consist merely in a re-crystallisation of the same mineral previously present in an amorphous or comminuted condition, without any true chemical action having necessarily been called into operation. When, however, instead of the beds being composed merely of one mineral, we find two of quite different nature—as, for example, mica and quartz—these two minerals, not being capable of reacting upon one another, except at such high temperatures as we have no reason to believe necessary for the development of foliation in rocks, usually segregate or separate from one another, so as to arrange themselves in more or less definite or distinct alternating layers, which, if the rock had been contorted or crumpled up by pressure, may often present the most fantastic appearances; as, for example, in the mica schists in Anglesea, a fragment of which is depicted in Pl. LXXIII., fig. 8.

Where one of the minerals has, for example, like garnet, a very great tendency to assume its crystalline form, we often find numbers of nearly, if not quite perfect dodecahedral crystals of this mineral, enveloped in the foliæ of mica or talc, which curve round them and compose the mass of the rock; the crystals of those minerals which possess an elongated crystalline form, like andalusite, actinolite, kyanite, rhœtizite, &c., arrange themselves, as a rule, lengthways between the layers of mica, chlorite, &c.

Many minerals which have less tendency to take the form of perfect crystals, appear, as it were, to segregate out in the form of lenticular or oval nodules, arranged with more or less regularity in the schist; a very perfect example of this is seen in the annexed woodcut, which represents a fragment of di-

Fig. 1.

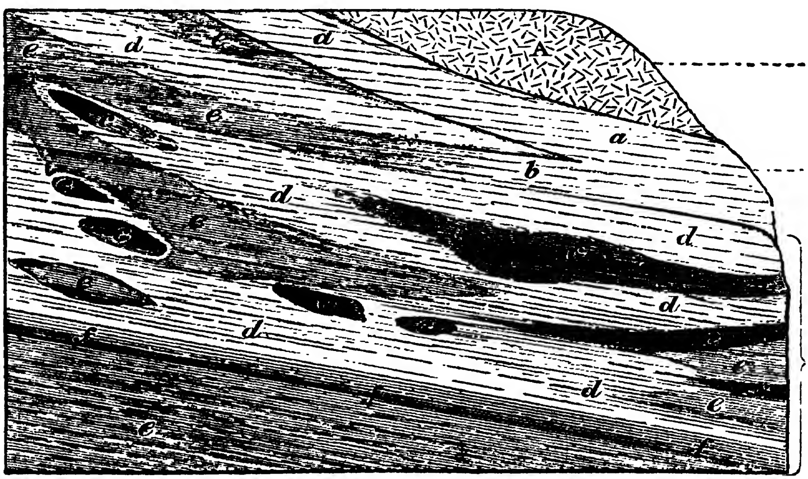


chroite schist, consisting of a mass of mica (with a little talc), enclosing innumerable nodules of dichroite of a whitish or bluish-white colour, sometimes exhibiting the characteristic play of colours. This specimen is from the borders of Ongsteens Vand in southern Norway, where this rock extends over a considerable area, and as the nodules are pretty uniformly of about the size of a walnut, and the foliation of the mica binds itself around them, the rock itself presents a very pecu-

liar appearance, from the immense number of the nodules and the regularity of their arrangement.

The more ancient limestones, formerly called primitive, which are usually more or less crystalline in texture, are frequently found to be foliated with chlorite or mica (as at Inverary), with augite and scapolite (as in the Hebrides), or with spinel and chondrodite (as at Christiansand, in Norway); other minerals might also be mentioned as inducing such structure in the metamorphic limestones. In some instances the foliation of different parts of the same large mass of crystalline limestone may be brought about by minerals very different in character to one another, as shown in the annexed figure.

Fig. 2.




This woodcut represents a section of a limestone quarry, near Christiansand, in Norway, in which A denotes the overlying granite; *a*, *b*, *c*, different varieties of crystalline schists in irregular patches; *d*, coarsely crystalline white limestone or marble; *e*, crystalline white limestone, foliated by small crystals of augite and scapolite; *f*, ditto, foliated by mica. Although in this section we find a somewhat confused arrangement of the different rocks, it is seen that the general direction of the lines of foliation remains constant, quite independent of the different character of the minerals by which they are expressed, or the nature of the rock which they traverse.

It must not be supposed, however, that the foliated structure in rocks is only developed by mineral silicates, as in the instances hitherto referred to; on the contrary, other and most distinct compounds frequently make their appearance: thus

when sedimentary strata, which have originally contained carbonaceous matter, have undergone metamorphic alteration, it not unfrequently happens that the carbon becomes converted into graphite, and thus gives rise to graphitic schists, which sometimes assume the exact appearance of ordinary mica schist, composed of alternate layers of quartz and mica; the latter mineral being in this case represented by the scales of graphite.

Metallic foliation, i.e. foliated structure developed by the occurrence of metallic oxides, sulphides, sulpharsenides, &c., is common in many countries, extending at times over considerable areas. Thus we find in Scandinavia, Brazil, India, and elsewhere, ferruginous schists, in which the mica is replaced by iron glance or the micaceous form of the sesquioxide of iron. The quantity of oxide of iron in these beds of iron schists, as they have been termed, sometimes increases so much as to preponderate over the stony matter, and form actual beds of iron ore often of great magnitude. In like manner we find, in several parts of Sweden, schists foliated with zincblende to such an extent as to be largely worked for zinc ore, as at Arkersund, Shyshyttan, Bovallen, &c.; whilst at Vena, in Sweden, and Modum, in Norway, similar schists occur (and are worked) containing cobalt ore and other arsenical compounds.

Beds of crystalline schists, foliated or impregnated with iron and copper pyrites and other sulphides, are common enough in many countries, and especially so in Scandinavia, where, from the rusty colour assumed by these rocks along the line of outcrop, they are called fahlbands.

With regard to the origin of the crystalline schists, the evidence seems in favour of the view that they are merely sedimentary beds of sand; arenaceous, micaceous, and other muds; and submarine tuffs of eruptive origin, altered by crystallisation, or what may be termed chemico-molecular action. Occasionally an examination under the microscope will reveal the contours of the original sand grains, and in some instances, as Sorby has shown, the existence of still unobliterated current structure, like ripple-drifts for example. The argillaceous shales and slates in some parts of Estramadura, in Spain, are seen to be converted into a variety of mica schist, when fragments are found surrounded by or enclosed in the granite. Some of the hornblende schists of southern Norway, which consist almost entirely of crystals of greenish-black hornblende, were long ago shown by the author to be the beds of tuff which had proceeded from the submarine eruptions of pyroxenite (highly augitic trap) in the vicinity, subsequently consolidated and re-crystallised *in situ*.

When, in addition to the mica and quartz, felspar is also present in these rocks, they receive the name of gneiss, or more

properly mica gneiss—a rock only differing from granite in possessing a foliated parallel structure: an attempt to give some idea of this structure is made in Pl. LXXIII., fig. 5, which is taken from a piece of ordinary gneiss. The origin of gneiss is still involved in much obscurity, and there can be no doubt but that some varieties of gneiss have a totally different origin from others. It is known that beds of true mica or hornblende schist, i.e. those composed only of hornblende or mica along with quartz, often become felspathic close to their junction with eruptive granites, so as to become lithologically gneiss, although petrologically they can only be termed felspathic schists, since by following up these beds they are soon found to be true schists.

Other and more characteristic gneiss, in which the felspathic element is inherent and much more prominent, has from old times, and with much show of reason, been regarded as formed from the débris of eruptive granites, due either to subærial disintegration or to their having been ejected into or under water, and thus converted into the condition of tuff, which, after having been arranged under water as sedimentary beds and consolidated, have become subsequently crystalline by metamorphic action, thereby causing them to assume the foliated appearance they now present.

Whatever may be the true origin of ordinary gneiss, there is, however, another variety of this rock, called granitic gneiss, which cannot have been other than eruptive granite originally, in which the parallel arrangement of foliation is a superinduced structure, developed in it subsequent to its solidification; in fact it is a metamorphic eruptive rock, just as the ordinary schists are but metamorphic sedimentary beds.

The proof that such granite gneiss was originally eruptive is seen in the disturbance which it has occasioned in the rocks through which it has broken, as also in the fragments of these rocks which it encloses; thus Pl. LXXIII., fig. 3, is taken from Darwin's admirable work on the Geology of South America, and represents a fragment, 7 yards long by 2 wide, of dark coloured rock with garnets in it, enclosed in the ordinary granitic gneiss of the country about Rio Janeiro, both being cut through by a still more recent small granite vein; fragments and patches of the other rocks are also seen in the granitic gneiss of Donegal and Galway in Ireland.

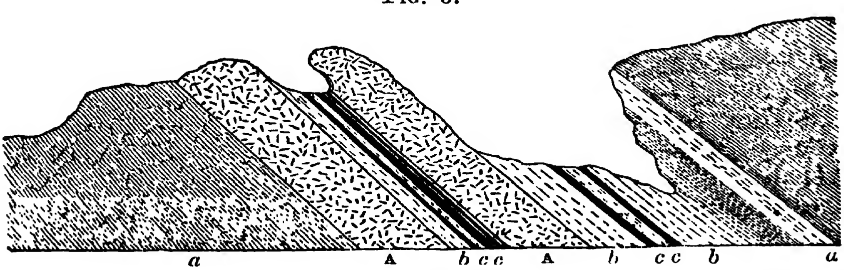
A magnificent section, several miles in length, of such granitic gneiss can be seen in the naked and almost perpendicular cliffs on the north side of Eidsvand, a lake in the south of Norway: a portion of this section, elsewhere published by the author in 1856, is represented in Pl. LXXIII., fig. 1, and shows the original dark hornblendic schists of the country broken through and

dislocated, whilst immense fragments of them are detached and enclosed in the mass of light-coloured granitic gneiss; and, as further evidence of its eruptive nature, fig. 6 shows how, at Haukeraadalen, this granitic gneiss throws out a small vein breaking through the adjacent hornblende schist and possessing all the structural characters of true granite, and entirely wanting the parallel lines of foliation seen dipping invariably at a high angle to the east in the main mass of granite gneiss. Similar cases have been described by Keilhau in other parts of Norway, and by Scott and Haughton in Donegal.

Even in the most characteristic eruptive granites, it is not at all uncommon to find portions of the rock possessing a more or less defined parallel structure, due to the position of the plates of mica in them, in consequence of which the rock, to use the workmen's language, "has a grain," and splits more easily in this direction—a feature which—as, for example, in some parts of the Aberdeen white granite quarries—is made use of for the purpose of making curbstones, &c. The lithologist, on seeing such stones, would describe them, and correctly so, as granitic gneiss, although petrologically—that is, studied *en masse* in the field—they are true granite. In Germany the distinction gneiss granite has been employed for such undoubted granites as possess a foliated structure.

The direction of the lines of foliation was for a long time regarded as that of the original stratification, and in many instances, no doubt, this is the case; for example, in the section

Fig. 3.




*a, a.* Ordinary gneiss. *b, b.* Foliated pink crystalline limestone.
*c, c.* Bands of augitic gneiss in the limestone.
A, A. Granite veins, each ten feet thick.

shown in the annexed woodcut, which is taken near Christiansand, and which shows alternating beds of ordinary and augitic gneiss with foliated crystalline limestone, which are without doubt the representatives of sedimentary beds of silicious and calcareous nature, originally deposited by water, and broken

through along the planes of stratification by the two granite dykes shown in the section.

Many years ago, however, attention was directed by Keilhau and others to the fact that the planes of foliation did not always coincide with those bounding the larger beds or masses of rock, seen to be distinct from one another by their differing in mineral composition, and that they were sometimes even at high angles to them. The subsequent observations of Darwin and Sharpe proved, in very different parts of the world, that, so far from being identical with stratification, the lines of foliation over large areas were also those of cleavage; and Mr. Sharpe, who regarded foliation as the final result of the same cause which induced cleavage in rocks, generalised, from observations in Scotland and the Alps, that the lines of these two structures, taken together, were parts of great arches many miles across—an hypothesis, however, which has not received subsequent corroboration. In 1854 the author announced, as the results of experimental as well as field observations, that the foliation in rocks appeared to be a structure induced subsequent to the consolidation of the rock, following the direction of the planes of least resistance in the rock, whether such planes were those of original sedimentary stratification, subsequent cleavage, or (in eruptive rocks) the striæ of fusion; and this view explains why the lines of foliation and cleavage so often coincide, since, if a rock had once undergone cleavage and subsequently became foliated, the foliations would naturally follow the planes of cleavage in preference to those of stratification, since the former would be those of least resistance.

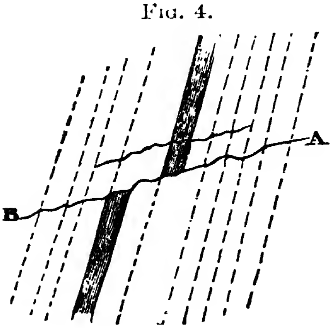
It is therefore of the utmost importance that geologists, when observing in the field, should—especially in districts consisting of metamorphic schists and gneiss—continually bear in mind that the planes of foliation may not necessarily be in any way connected with those of sedimentary deposition, and that, in such districts, the only means of arriving at any sound conclusion as to what the probable original bedding had been, is by carefully studying the difference in mineral character of the various rock masses superposed one on another.

What the cause of foliation may be, is a problem as yet but little investigated or understood. Heat (not necessarily intense) appears certainly to have played an important part, since foliated rocks are rarely or ever met with unassociated or at any great distance from rocks of eruptive, i.e. of igneous origin. Experiments made by the author between 1849 and 1853 showed, when blocks of massive or amorphous soapstone were exposed for some months to a temperature not exceeding redness (infinitely below what would be sufficient to fuse or even

soften the rocks), under a slight pressure of from about seven to twelve pounds per square inch, that they became totally changed in structure and converted into an aggregate of finely developed crystalline foliæ of a brilliant white or greenish colour, identical with talc; in fact, a talc schist. Under similar circumstances, if protected from oxidation—as they contain some iron in the state of protoxide—ordinary clayslates were converted into rocks possessing a beautiful parallel structure, resembling gneiss so closely, that some of the hand specimens could not be distinguished by the eye from parts of the same clayslate altered at its points of contact with eruptive rocks in nature. In the first of these cases, mere re-crystallisation or molecular re-arrangement in the solid mass will explain the change; but, in the second, chemical action also has evidently come into play in re-arranging the chemical elements of the clayslate into other mineral forms not pre-existing in the slate. The effect of the heat being to expand and render the pores of the rock more open, doubtless admits of the molecules re-arranging themselves and crystallising in the still solid rock, whilst at the same time the superincumbent pressure tends to force the crystals to shoot out or develope themselves in one direction only, i.e. at right angles to the pressure.

These experiments, and the formation of the well-known Réaumur's porcelain, show how such crystalline structure can be developed in solid bodies after their perfect solidification without any return to a fluid or molten condition; and the annexed woodcut, taken from Keilhau, which represents an appearance in the gneiss of Jomfrueland, an island on the southern coast of Norway, can only be explained on the assumption that the direction of the lines of foliation in the rock (which are represented by the dotted lines) had been determined subsequent to its complete consolidation, since it will be noticed that the subordinate bed of dark hornblendic character has been dislocated by the fault A B, without any corresponding disturbance in the lines of foliation.

Fig. 4.




In the present state of science it would be premature to say more as to the probable causes of foliated structure; the hope may, however, be expressed, now so much attention is devoted to geological research, that the study of this interesting although abstruse subject may no longer be neglected.

*Striation*, or that structure which is due to the presence of what are termed the striæ of fusion, has frequently been called

slag, glass, or lava structure, from its being so characteristic of these substances also.

Everyone has probably noticed the existence, especially in glass of inferior quality, of certain lines which more or less injure the transparency of the glass, although in themselves quite transparent. In the portions of glass remaining attached to the bottom and sides of old or broken glass pots these lines are still better seen, being commonly rendered distinctly visible by alternating lines or layers of glass possessing different tints of green with others white or colourless, and often presenting a parallel structure of great beauty, especially when these lines are seen to be contorted and crumpled up into all manner of shapes. The same structure is seen in the vitreous slags from iron furnaces, the colours being usually shades of blue, yellow, green, and grey; whilst the slags from the copper-smelting furnaces often show extremely beautiful alternating striations of a deep red and black colour.

In obsidian or the so-called volcanic glass, an exactly identical striped or banded structure is extremely common, and in Pl. LXXIII., fig. 7, which represents a piece of obsidian from the Lipari Islands, an attempt has been made to convey an idea of this appearance.

This structure appears to be caused by the movements of the different parts of a viscid molten mass, which flow over one another at different rates of progression, whilst its existence is usually denoted by the bands or stripes in the rock either differing in colour or in the shades of some one colour. Sometimes, especially in obsidians and traps, this structure is accompanied by an innumerable number of small air-bubbles (? gas or steam also), which, being drawn out or elongated from their original spherical shape by the progressive movement of the molten mass, develop a parallel structure of very peculiar appearance. In the older lavas these cavities often become, through infiltration, filled up with carbonate of lime or other minerals, and thus form what is termed amygdaloidal trap or lava.

When such lavas, glasses, or slags cool quickly, they retain their vitreous character and the striated structure already described; when, however, the cooling takes place very slowly, or when, by natural or artificial agencies, they have been again heated and kept so for a considerable time, devitrification commences to take place, the crystalline or stony structure developing itself first along the lines of striation. In Pl. LXXIII., fig. 2, which depicts a section of a fragment of greenish plate-glass from the St. Helen's works, an attempt has been made to illustrate the gradual development of crystallisation (or foliation, in other words) along the lines of striation, origi-

nally invisible or but faintly distinguishable, when the glass had been for some time exposed to a low heat in the flue of the annealing oven. Had this specimen been kept long enough in the hot flue, it would eventually have become altogether devitrified or converted into a crystalline stony mass (Réaumur's porcelain), in which, however, the parallel structure due to the striæ of fusion can still be distinguished either by the naked eye or under the microscope, owing to the general direction and parallelism of the longer axes of the crystals which compose the mass.

What is thus artificially produced on the small scale can be seen on the large scale in nature in obsidian and other glassy lavas; and in most volcanic countries hand specimens can be obtained showing all the stages, exactly as in the case of glass. The crystals of the mineral components of the lavas appear first along the lines of striation, and go on forming until the whole mass has become devitrified and is entirely crystalline, and resembles Pl. LXXIII., fig. 9, which represents a hand specimen from the apparently bedded and intercalated lavas (which form so peculiar a feature of this part of South America) near the river Mauri, where it forms the boundary-line between the republics of Peru and Bolivia. In this specimen the rock has lost all trace of its originally glassy or vitreous appearance, and having become completely devitrified, is seen to consist mainly of felspar crystals along with a little augite, quartz and an occasional plate of mica (a trachydolorite); yet, as is seen in the figure, a distinct parallel structure (due to the striæ of fusion) is still preserved in the rock, which, being the lines of least resistance in it, renders it much more fissile in this direction than across the grain, exactly as is commonly found to be the case in a normal or metamorphosed rock of true sedimentary origin: in fact, some of these rocks, which underlie conformably oolitic and other strata of still later age, and which show themselves for miles intercalated conformably in the strata, have been mistaken for true sedimentary beds, and in one instance described as sandstone (which never could have occurred had their mineral nature been examined), although they are easily traced to the active volcanoes in the vicinity from which they have been emitted. Even when such rocks are found to be extremely fine-grained in texture, the parallel structure due to pre-existing striation, although often very difficult to distinguish on the rough surface of fresh fracture, is commonly found to show itself very distinctly on the surface of the rock when weathered.

Any geologist who has had the opportunity of studying such rocks in the field, cannot but consider it probable that the so-called granitic gneiss or gneiss granites owe their structure to milar causes.

In concluding these remarks on the different classes of parallel structure met with in rock masses in nature, the author would but state that his main object has been to direct the special attention of geologists working in the field to the extreme importance of studying carefully, not only the external contours, but the internal structure of rock masses also, in order thereby to avoid the mistakes which have been frequently committed; such as recording as stratification planes which are in reality due to the effects of joints, cleavage, or foliation, or describing rocks of truly intrusive or eruptive nature as of sedimentary origin.

# BRITISH BEARS AND WOLVES.

By W. BOYD DAWKINS, M.A., F.R.S., F.G.S.

THE lion, mammoth and reindeer, have already been discussed from the point of view offered by palæontology and archæology. In the present essay I intend to treat of the value of bears and wolves in classification, and to see how far they throw light on the ancient physical condition of Britain. In the latter respect we shall find that they offer testimony to the state of things, which is of no small importance to the student of early English and mediæval history, while in the former they compel us to analyse M. Lartet's method of subdividing the quaternary period.

The *genus* bear has not been discovered in any deposits of greater antiquity than the pleiocene age, and is represented in Europe by many extinct species and several existing varieties. The British species are four in number: the bear of Auvergne, the cave and grizzly, and the common brown or black bear. Each species has its own peculiar range, both in space and in time. The first, or the *Ursus Arvernensis*—the remains of which are preserved in the magnificent collections from the forest-bed of Norfolk made by the Rev. J. Gunn and the late Rev. S. W. King—was a creature of about the same size as the common European species, and armed with canines which are quite puny in comparison with those of the *Ursus spelœus*. It has not yet been figured or described as a British species, and its remains are very rare. On the Continent, however, it is, as Dr. Falconer remarks, abundantly found in the pleiocenes of Auvergne and of the Val d'Arno. It has not yet been found in any of the German pleiocenes, nor in any strata younger than the preglacial forest-bed of Norfolk. We may therefore view it as a pleiocene carnivore of a southern kind, which ranged from Northern Italy, through France, as far north as Norfolk, before the lowering of the temperature during the glacial epoch, in company with several of the pleiocene species, such as the *Cervus ardeus*, *Rhinoceros megarhinus*, *R. etruscus*, *Elephas meridionalis*, and *Trogontherium Cuvieri*. The whole group of animals besides the above, associated with the *Ursus Arvernensis* in Britain, is as follows:—

*Mygale Moschata.* The musk-shrew.
*Talpa Europœa.* The mole.
*Cervus Capreolus.* The roe.
*C. Elaphus.* The stag.
*Bos primigenus.* The urus.
*Hippopotamus major.*
*Equus fossilis.* The horse.
*Elephas antiquus.* The narrow-toothed elephant.
*Arvicola amphibia.* The water-rat.
*Castor fiber.* The beaver.

It is worthy of note that all the members of this fauna now alive are to be found in temperate regions, and there is every reason to believe that the extinct members also rejoiced in a temperate or comparatively warm climate. On the whole, we may predicate a southern range of the *Ursus Arvernensis*, just as in the case of two at least of the other British species we can predicate a northern origin. It defines with as much sharpness as can be hoped for in palæontology the pleiocene horizon of any strata in which it occurs, and particularly the stage immediately before the refrigeration of Central Europe had brought in the Arctic mammalia, and forced down southwards the pleiocene fauna of Britain, France, and Germany.

We must now pass on to the consideration of the cave-bear, *Ursus spelœus*, the remains of which were among the first to be assigned to their true owners by the naturalists of the eighteenth century. The use of bones in medicine,* in the

* At the present day the Chinese are in the habit of using fossil-bones in medicine; and within the last few years the bone-caves of Borneo have been ransacked for the same purposes as the caves of the Hartz in the seventeenth and eighteenth centuries. To such a degree has this been carried, that up to the present time no European has been able to transmit home a collection of fossil mammalia from Borneo, because of the high price which the Chinese demand. The specimens described by Professor Owen from China, in the "Quarterly Geological Journal" for 1870, were bought by Mr. Swinhoe from Chinese who had collected them for physic. In the Chinese "Materia Medica" they are described under the head of dragons' teeth. Even human remains were commonly used in Britain in medicine during the seventeenth century. In the days of Sir Thomas Browne, "celestial mummice," or pulverised mummies, was commonly imported into Britain from Egypt; and it appears that even the tumuli round Abury were not safe from the incursions of eminent doctors.

In 1670 Dr. Robert Toope, a physician, then resident at Marlborough, in a letter to John Aubrey, gives a curious relation of the discovery, by labourers, of skeletons at this place (Abury), which, he says, had the name of Millfield. Dr. Toope terms the double circle a "temple," and describes it as "a large spherical foundation, whose diameter is forty yards; within

sixteenth, seventeenth, and eighteenth centuries, caused the bone-caves of Saxony and Bohemia to be eagerly explored by the searchers after *Album græcum*, which in the old pharmacopœias did not merely mean elephant's tusk or hyæna's coprolite, but any fossil-bone whatsoever. And as among the remains obtained from the caves those of the cave-bear were very abundant, the similarity which they bore to the animal living in Germany led to the recognition of their true nature, and they were gradually withdrawn from the province of medicine into that of palæontology, under the name given to them by Dr. Goldfuss. During the latter part of the eighteenth and the first quarter of the nineteenth centuries the range of the cave-bear was gradually extended, by various discoverers of its remains, from Germany into France, and lastly, by the famous exploration of Kirkdale by Dr. Buckland, into Britain. It was discovered by the Rev. J. M'Enery, in Kent's Hole, and subsequently in no less than twenty out of the thirty-six British post-glacial caves, the contents of which I have tabulated in the "Quarterly Geological Journal," 1869.

In Belgium, the limestone caverns around Liège yielded large quantities of its remains to Dr. Schmerling, the great rival of Dr. Buckland in cave-hunting. Recent investigations have shown that it crossed the Alps into Lombardy, and it has been met with also in Southern Russia. Its range in space may be said to extend from Yorkshire and Liège in the north, through Germany and France as far as the plains of Lombardy. It probably also found its way still further south in Italy, no geographical barrier intervening, although M. Cesselli's quotation of it from the gravel-beds of Rome has not as yet been verified. It has not yet been discovered in Northern Germany or Scandinavia, or in Northern Siberia, where the vast accumulation of fossil-bones have excited the curiosity of the most eminent naturalists, such as Pallas and Brandt. Its absence

this there is another orb whose sphere is fifteen yards in diameter; round about this temple a most exact playne; and but little more than a foot under this superficies laid the bones soe close one by another that scul toucheth scul. I exposed two or three, and perceived their feet lay toward the temple; and I really believe the whole ground is full of dead bodies." He adds that the bones were large, but much decayed, though "the teeth were extreem and wonderfully white, hard, and sound;" upon which he notes: "no tobacco taken in those days." Dr. Toope says: "I came the next day and dug for them (the bones), and stored myself with many bushels, of which I made a noble medicine, that relieved many of my distressed neighbours." Aubrey adds: "This was in 1678, and Dr. Toope was lately (1685) at the Golgotha again, to supply a defect of medicine he hath had from hence."—"Crania Britannica, Ancient British," Kennet, vol. ii.

can hardly be ascribed to the fact of the fossil mammals of those regions having been ignored, for they have been studied and described with the minutest care. It is numerically most abundant in the caves of Franconia. The cave of Kuhloch, for example, which in size is equal to the interior of a large church, contained, according to Dr. Buckland, "hundreds of cartloads of black animal dust, entirely covering the whole floor to a depth which would average at least six feet. . . . . . The quantity of animal matter accumulated on this floor is the most surprising, and the only thing of the kind I ever witnessed; and many hundred, I may say many thousand, individuals must have contributed their remains to make up this appalling mass of the dust of death. It seems in great part to be derived from comminuted and pulverised bone; for the fleshy parts of animal bodies produce by their decomposition so small a quantity of permanent earthy residuum, that we must seek for the origin of this mass principally in decayed bones. The cave is so dry that the black earth lies in the state of loose powder, and rises in dust under the feet."* Dr. Buckland estimates the whole mass of animal matter in this cave at 5,000 cubic feet, which, at the too liberal estimate of two cubic feet of matter for each animal, would involve the presence of 2,500 bears. The dryness of the cave, and the freedom of the animal matter from loam or other admixture, call to mind the condition of the Egyptian sepulchres rather than an ordinary bone-cave; and, so far as I know, have been observed only in this particular instance. In the rest of the German caves, as in the English, the animal remains are embedded in a red or grey loam or sandy clay, introduced either by streams or from the gradual drip of the water from the roof. The enormous preponderance of the remains of cave-bear over those of the associated mammalia proves that Germany was the head-quarters of the cave-bear, whence it may be said to have passed to the south and to the west, but not to the north.

The value of the animal in classification, and its range in the geological past, are questions of considerable difficulty, which demand a most careful analysis. On the Continent it has not, as yet, been recognised among the animals from any pleiocene strata, and in our own country it has been obtained only from one deposit of pre-glacial age, namely, the Lacustrine deposit at Bacton on the Norfolk shore, which underlies the boulder clay. During the post-glacial epoch it was associated with the mammoth, reindeer, cave-hyæna, and cave-lion, and the whole suite of animals which compose the fauna of the period. Its remains are, however, distributed very un-

* "Reliquiæ Diluvianæ."

equally, being found only in the British caves, and not in the river deposits, which were probably of the same geological date. This peculiarity of distribution may be attributed to the habit of living in caves which characterised the creature, in common with the hyæna, and not, as M. Lartet supposes, from the creature not being here at the time the river-deposits were accumulated. But, however this may be, the creature passed away from France, Germany, and Britain at the close of the quaternary or post-glacial age, and probably also from North Italy, since the occurrence of its remains in a cave which furnished also a polished stone-axe and pottery does not necessarily imply that it lingered there as late as the Neolithic age. Thus we have evidence that it appeared in Europe just before the glacial epoch, at the close of the pleiocene, and increased and multiplied during the post-glacial or quaternary age, at the close of which it became extinct.

Thus far the evidence of the geological range of the animal is plain enough, but the eminent French palæontologist, whose loss is so much to be deplored, M. Lartet, has attempted a scheme of classification by means of the cave-bear and other animals, which, as it seems to me, has been accepted by naturalists without sufficient grounds. He divides the quaternary series into four periods: "L'âge du grand ours des cavernes, l'âge de l'éléphant et du rhinocéros, l'âge du renne, et l'âge de l'aurochs.' The very simplicity of the system has made it popular. You find a cave-bear, and at once you have a date for the deposit in which it occurs; you find an aurochs, and at once assign the stratum to the latest stage of the quaternary. Unfortunately, however, there are two objections, either of which is fatal to this mode of classification. In the first place, nobody could expect to discover the whole quaternary fauna buried in one spot. One animal could not fail to be better represented in one locality than in another, and therefore the contents of caves and river deposits must present some variation. The den of a hyæna could hardly be expected to contain exactly the same animals as a cave which had been filled with bones by the action of water. It therefore follows that the very diversity which M. Lartet insists upon as representing different periods of time must necessarily have been the result of different animals occupying the same area at the same time. In the second place, M. Lartet has not advanced a shadow of proof as to which of these animals was the first to arrive in Europe. It is undoubtedly true that they died out one by one, and it is very probable that they came in also gradually. The fossil remains from the English caves and river deposits—as for instance those of Kent's Hole, or Bedford—prove only that the animals inhabited Britain at the same time, and do not in the

least degree warrant any speculation as to which animal came here first. When the French and Belgian naturalists have tabulated all the animals found in their respective countries, they may generalise freely about the absence, or presence, or preponderance, of certain species. Up to the present time this has not even been attempted by any writer on the subject. In the caves of Britain—such as Kent's Hole, Wookey Hole, Kirkdale, and the rest, which may be seen in my table of distribution (*British Post-Glacial Mammals*, "Quarterly Geological Journal," 1869)—the cave-bear is associated indifferently with all the three animals which M. Lartet selects as being of classificatory value. In a word, all that we can safely say of the post-glacial range of the cave-bear is that it is found in a great many of the caves in such association with the other mammalia that it cannot be considered characteristic of any of the stages into which the post-glacial age has been arbitrarily divided. We cannot tell from what area it migrated into Europe; but, from its known range over the central and southern portions of the Continent, we may assume that it came from a comparatively temperate region, and not from the present home of the reindeer or musk-sheep. It very probably came from Southern Siberia. If, indeed, we allow that the severity of the glacial period began in the present northern regions, and gradually increased, the arctic mammalia would gradually encroach on the temperate zone, and compel the dwellers in that zone to retreat further and further southwards. The necessary result of the glacial cold would be a boulversement of geographical provinces, such as we find at the close of the pleiocene, and during the post-glacial age. If this view be accepted, the presence of the cave-bear in Britain at the close of the pleiocene may be ascribed to the increase of cold in the northern regions.

The grizzly bear, *Ursus ferox*, has been proved by Professor Busk to be identical with the species described by Dr. Goldfuss under the name of *Ursus priscus*, and is comparatively common in the British post-glacial caves and river deposits. It occurs in the lower brick-earth deposits on both sides of the Thames, at Crayford in Kent, and Ilford and Grays Thurock in Essex, along with *Elephas antiquus* and the megarhine species of rhinoceros, and is more abundant in the British caves than its spelæan congener. On the Continent its remains are found in the caves of Belgium and Germany, and in France it has been described by M. Lartet under the name of *Ursus Bourguignati*. It also probably occurs in Italy, according to the opinion of the late Dr. Falconer.* Its range in Europe, therefore, may be said roughly to coincide with that of the cave-

* "Palæontographical Memoirs," vol. ii.

bear. It has, however, not been found in any pre-glacial deposit north of the Alps, but is strictly a post-glacial creature in France, Germany, and Britain. Like the cave-bear, it vanished away before the dawn of the pre-historic age. At the present day, according to Sir John Richardson,* it inhabits the region of the Rocky Mountains and the plains lying to the eastward, as far north as latitude 61°, and, according to Lieut. Pike, as far south as Mexico. It must have retreated to its present abode by the same route as its arctic fellow-inhabitant of Great Britain in post-glacial times, the musk-sheep, eastward over the great plains of Siberia, and over the straits of Bheering. The massive skulls of the musk-sheep scattered through the post-glacial strata of France, Germany, and Russia, and preserved in the frozen gravels of Northern Siberia, and lastly, in the ice-cliff in Eschscholtz Bay, on the American side of Bheering's Straits, point out unerringly the continuity of land, and the direction in which the migration took place.

The common European bear, *Ursus Arctos*, is the fourth and last of the species which have inhabited England during the geological past, and it is of considerable interest, because it is the largest of the post-glacial carnivores which can be brought into relation with our history. It occurs but sparingly in the post-glacial deposits of Great Britain, and was by no means so abundant as the two other post-glacial species. It has not yet been discovered in any pleiocene deposit, nor has it excited the attention of the foreign palæontologists, and consequently we cannot tell its ancient Continental range. In Britain it survived those changes which exterminated the characteristic post-glacial mammalia, and is found in the pre-historic deposits both in Great Britain and Ireland. It is described by Professor Owen from the marls underneath the peat of Manea fen, in which also reindeer, the Celtic shorthorn, and horse, have been found, and is mentioned by Mr. Scott as having been discovered both in Longford and King's County. ("Catalogue of Mammalian Fossils discovered in Ireland," Geological Society, Dublin, February 10, 1864.) It became extinct in Ireland probably before the historic period, for according to St. Donatus, who died in 840, in that favoured island, "Ursorum rabies nulla est ibi." Sir William Wilde mentions, however, that there is an Irish name for the animal in an old glossary in the library of Trinity College, and Thompson mentions traditions of the existence of the animal. (Mr. Scott, op. cit.)

The recent exploration of the Victoria cave, near Settle, has revealed the fact that the *Ursus Arctos* formed a portion of the food of the Neolithic dwellers in the cave, who have left the

* "Fauna Boreali-Americana."

relics of their feasts and a few rude implements at the lowest horizon. The brôken bones and the jaws of the animal lie indiscriminately mixed up with the remains of the red-deer, horse, and Celtic shorthorn (*Bos longifrons*). Two of the jaws, indeed, rival in size those of the great cave-bear. The section at the entrance of this cave shows the relation in point of time between the Neolithic occupation and the present day. On the surface is an accumulation of angular fragments of limestone, fallen from the cliff, which rests on a stratum two feet in thickness, containing Celtic enamels and Roman coins, which cannot be of a later date than A.D. 746. Under the latter is a similar accumulation, no less than six feet thick, which rests on the lower Neolithic horizon. It may therefore fairly be assumed to have taken thrice as long a time for its accumulation as the first—that is to say, if 1,200 years be required for the first, 3,600 years would be required for the second. And thus we roughly get at the date when the bear was eaten by the Neolithic hunters, which would be about 4,000 years ago. This is one of the very few cases in which even a guess can be hazarded of the lapse of past time in the region outside history.

Nor are we without direct testimony that the bear was killed by the hand of man during the Roman occupation of Britain. In the collection of bones from the refuse heaps round Colchester, made by Dr. Bree, the animal occurs along with the badger, wolf, Celtic shorthorn and goat. I have also met with it in a similar refuse heap, at Richmond in Yorkshire, which most probably is of Roman origin. Indeed, in the northern parts of our island, the bear was sufficiently abundant during the Roman occupation to have been exported to Rome for the gladiatorial shows, unless Martial's "Ursus Caledonius" be a mere flight of poetical imagination.

> Nuda Caledonio sic præbuit pectora urso
> Non falsâ pendens in cruce Laureolus.

The mention of a trade in Caledonian bears in Plutarch goes far to prove the truth of this incidental allusion. But whether it be true or not, there can be but little doubt that the animal inhabited the great Caledonian forest during the Roman occupation of Britain. The precise date of its extinction cannot be determined with any very great accuracy. One of the Gordons is said to have killed the last bear in Scotland in the year 1057, but I have been unable to verify the fact. The statement is made by Pennant, on the authority of an heraldic legend. The animal might naturally have been expected to have lingered in the mountains of Wales long after it was driven from the cultivated and fertile portions of Roman Britain.

On the authority of Dr. Ray*, it is stated to have been one of the beasts of the chase in North Wales, and is believed by Pennant to have left traces of its former presence in the name Pennarth, or bear's head. But however this may be, the animal doubtless became extinct in our island before the close of the eleventh century.†

The history of the sojourn in Europe of the *Ursus Arctos* is, as we have seen, clearly ascertained. The animal made its appearance in Britain after the glacial epoch had passed away, and lived on uninterruptedly down to the present day. The fact that it disappeared before the tenth century from Britain, while it still held possession of considerable areas in France and Germany, implies that the state of agriculture here was higher than that of those two countries at the time. It could not exist without long stretches of uncultivated lands. At the time of the Norman conquest huge forests overshadowed a considerable portion of Great Britain, to an extent which as yet has not been properly realised by any historian, but nevertheless the hunter at that time had so completely ransacked their recesses, that he had extirpated the bear, and thus prepared the way for the farmer. At the present day it lives on most of the high mountains of Europe, but is annually becoming more scarce. In 1852, according to Lord Clermont, five were seen together in the Engadine. The animal ranges throughout Northern Siberia, and is probably represented by two varieties in North America.

The following table represents the range in line of all the four British species.

| | Pre-glacial | Post-glacial | Pre-historic | Historic |
|---|---|---|---|---|
| *Ursus Arvernensis* . . | × | — | — | — |
| *U. spelæus* . . . | × | × | — | — |
| *U. ferox* . . . . | | × | — | — |
| *U. Arctos* . . . | | × | × | × |

* Raii, Syn. Quad. 214.

† The reputed efficacy of bear's grease in "strengthening the roots of the hair" may perhaps be a superstition that points back to the time when the animal was hunted in Britain. It was an important ingredient in many kinds of ointment in the Middle Ages, and, mixed either with the burnt head of a *hare*, or a burnt mouse and honey, was supposed to cure baldness. (See "Gesner, Nat. Hist.," folio, vol. i. p. 949). It is very remarkable that the bear should be singled out as ranking far higher than the rest of the beasts of the chase, and as an object of reverence, throughout nearly the whole of the northern Euro-Asiatic continent, and the northern regions of America. The Laps make almost a religious ceremonial of going out to the bear-hunt and the return ("Lapponia, Scheffer," Frankfort, 4to. 1672), and carefully bury the bones of the bear. The Tunguzes of the Amoor treat it, when slain, with religious respect, and the Red Indians ask pardon of the dead animal for being compelled to kill it.

The Polar Bear (*Ursus maritimus*) is believed by Dr. Carte to have been an inhabitant of Ireland in ancient times, on the evidence afforded by some bones found in Lough Gur, in Limerick. The proportions, however, of the long bones and the position of the third trochanter on the inner side of the femur, differentiate the ursine bones in question from those of the polar-bear. They resemble the cave-bear more than any other; but the variations presented by the remains of the genus *Ursus* are so great that in many cases it is impossible to determine the species. The size is of no value in differentiation, because in the post-glacial and pre-historic times—before man had seriously entered into competition with the carnivores—the wild animals were as a whole much larger than at the present day, when they are driven from the areas where food is abundant. The recent specimens in most of the museums afford, therefore, no true guide to specific determination so far as relates to variation in size. The genus bear also, in common with the hippopotamus and wild boar, present greater variations in form than most of the contemporary genera, and in very many cases I have been unable to assign the remains to its rightful owner. And that this feeling is shared by other naturalists, is shown by the appalling lists of fossil bears, which merely are the expression of the impossibility of assigning them to any one well-known form.

We must now pass on to the history of the wolf in Britain. During the post-glacial age the animal was rare as compared with the hyænas and the bears, and its remains occur both in caves and river deposits. It has also been obtained from the pre-historic peat-bogs and alluvia in various parts of Great Britain. The only case within my knowledge of its remains being associated with the traces of the Romans is afforded by the collections from Colchester, made by Dr. Bree, and already mentioned. After the English invasion, and the populous Roman province of Britain had been devastated with fire and sword, wolves increased to such a degree that they are deemed worthy of notice in the public records. At Flixton, near Filey, in Yorkshire, writes Camden,* an hospital was built in the time of Æthelstan for defending travellers from wolves (as it is word for word in the public records), that they should not be devoured by them. In the reign of one of his successors, Eadgar, we hear of a tribute of three hundred wolves' heads on Judwal, a prince of Merioneth, who, according to William of Malmesbury, paid this tribute for three years, and desisted on the fourth, because he could not find one more.† He very

* "Camden's Britannia," edit. Gibson, vol. ii. p. 110.

† "Wil. Malm." ii. 155, vol. i. p. 251, edit. Hardy.

likely drove them out of his territory, but they still continued numerous in other quarters. The statement in the metrical account of the battle of Hastings, that Duke William collected and buried his own slain, while he left the English a prey to the birds and wolves,* is probably literally true, because the tangled thickets of the Andredsweald must have been the lurking-places of wolves at the time. In 1281 they had increased so much in the counties of Gloucester, Worester, Hereford, Shropshire, and Stafford, that one Peter Corbett was ordered by Edward I. to destroy them by any means that he could.† This is the last historical notice which I have been able to verify. Camden, however, mentions them as formerly infesting the Peak country, and there is said to be preserved at Exeter a record in which they are mentioned as infesting Devonshire. Taking everything into consideration, it seems very probable that they were exterminated in England and Wales before the end of the fourteenth century; for had they been present in any force, their ravages would certainly have been placed on record. Their memory is preserved in several names of places in different parts of England. "The spacious palace," for example, called Wolvesey, built close to the east side of Winchelsea Cathedral by Bishop Henry in 1137, and Wolvey, near Nuneaton, where Edward IV. was taken prisoner by the Earl of Warwick, after the battle of Dane Moor, may be quoted as examples.

The wolves lingered some time longer in Scotland, as might be expected from the country affording better cover than in England. The last wolf in Scotland was killed, according to Pennant, by Sir Ewen Cameron in the year 1680.

The wolf also lived in Ireland during the pre-historic period,

* "De Bello Hastingensi Carmen," by Guido, Bishop of Amiens:—

"Lustravit campum tollens et cæsa suorum
Corpora, Dux, terræ condidit in gremio;
Vermibus atque *lupis*, avibus canibusque voranda
Deserit Anglorum corpora strata solo."

† "Rymer Fœdera," folio, Lond. 1705, p. 168. 1281 An. 9 E. 1. "Rex omnibus Ballivis, etc. Sciatis quod injunximus dilecto atque fideli nostro Peter Corbet quod in omnibus Forestis et Parcis, et aliis locis infra comitatus nostros Gloucestr. Wygorn. Hereford. Salop. et Stafford. in quibus lupi poterunt inveniri, lupos cum hominibus, canibus et ingeniis suis capiat, atque destruat modis omnibus quibus videntur expedire.

"Et ideo vobis mandamus quod eidem Petro in omnibus, quæ ad captionem luporum in comitatibus prædictis pertinent, intendentes sitis et auxiliantes, quotiens opus fuerit et prædictus Petrus vobis scire faciet ex parte nostrâ. In cujus etc. duratur quamdiu nobis placuerit. Teste Rege apud Westminst. decimo quarto die Maii."

and its ravages have caused its existence to be placed on record after the English Conquest. Dr. Scouler has collected nearly all the notices on the point (Journ. Geol. Soc. Dub., vol. i. p. 225). "Great numbers of wolves formerly existed in Ireland, and they maintained their ground in this country for a longer period than in any other part of the empire. Campion, whose history of Ireland was published in 1570, informs us that wolves were objects of the chase. 'They (the Irish) are not,' he says, 'without wolves, or greyhounds to hunt them, bigger of bone and limme than a colt.' A century later they appear to have been equally abundant, for we find by the journals of the House of Commons that, in 1662, Sir John Ponsonby reported from the Committee of Grievances that a Bill should be brought in to encourage the killing of wolves and foxes. Effective measures for this purpose appear to have been taken, and the wolf was at last extirpated about the year 1710. Dr. Smith, in his 'History of Kerry,' when speaking of certain ancient enclosures, observes that many of them were made to secure cattle from wolves, which animals were not finally extirpated till the year 1710, as I find by presentments for raising money for destroying them in some old grand jury books."

According to Mr. Thompson,* three Wolf-hills in Ireland claim their name from the killing of the last wolf—"one in the south, another near Glenarm, and the third three miles from Belfast."

The mischief done by these destructive creatures may be estimated by the various orders relating to them, which have been extracted from the Council Books preserved in Dublin by Mr. Hardiman.† In 1652 the export of "wolfe dogges" was declared to be illegal by Cromwell's government, "because the wolves doe much increase and destroy many cattle." In the following year, also, in the preamble of a "Declaration touchinge the Poore," it is stated that "Many times poore children, who lost their parents or deserted by them, are found exposed to, some of them fed upon, by ravening wolves and other beasts and birds of prey." This increase of the wolves is directly traceable to the devastated condition of Ireland after the rebellion had been ruthlessly stamped out by Cromwell. In France the wolves have already taken advantage of the desolate state of the country after the war to find their way to the battle-fields near Amiens. In the same year there is a third declaration ordering the destruction of wolves, to which

* "Nat. Hist. of Ireland," vol. iv. p. 34.

† "A Chorographical Description of West Connaught," by Roderic O'Flaherty, edit. James Hardiman, Dublin. Note D, p. 180.

the general extirpation of the wolf in 1710 may be directly assigned: "And it is further ordered that all such person or persons who shall take, kill, or destroy any wolfes, and shall bring forth the head of the woulfe before the said commanders of the revenue, shall receive the sums following, viz.: for every bitch wolfe, six pounds; for every dogg wolfe, five pounds; for every cubb which preyeth for himself, forty shillings; for every suckling cubb, ten shillings; and no woolfe, after the last of September until the 10th of January, be accounted a young woolfe; and the Commissioners of the Revenue shall cause the same to be equallie assessed within their precincts.'

Such is the history of the British wolf. The date of its disappearance from England, Scotland, and Ireland affords a clue to the physical condition of those countries at the time. The hunter destroyed the only carnivores formidable to the shepherd and husbandman in England before the close of the fourteenth century; in Scotland in 1686, and in Ireland in 1710; and the two survivors—the bear and wolf—of the band of larger beasts of prey that dwelt here in the post-glacial age, finally disappeared from Great Britain and Ireland.

# THE "LOTOS" OF THE ANCIENTS.

By M. C. COOKE, M.A.

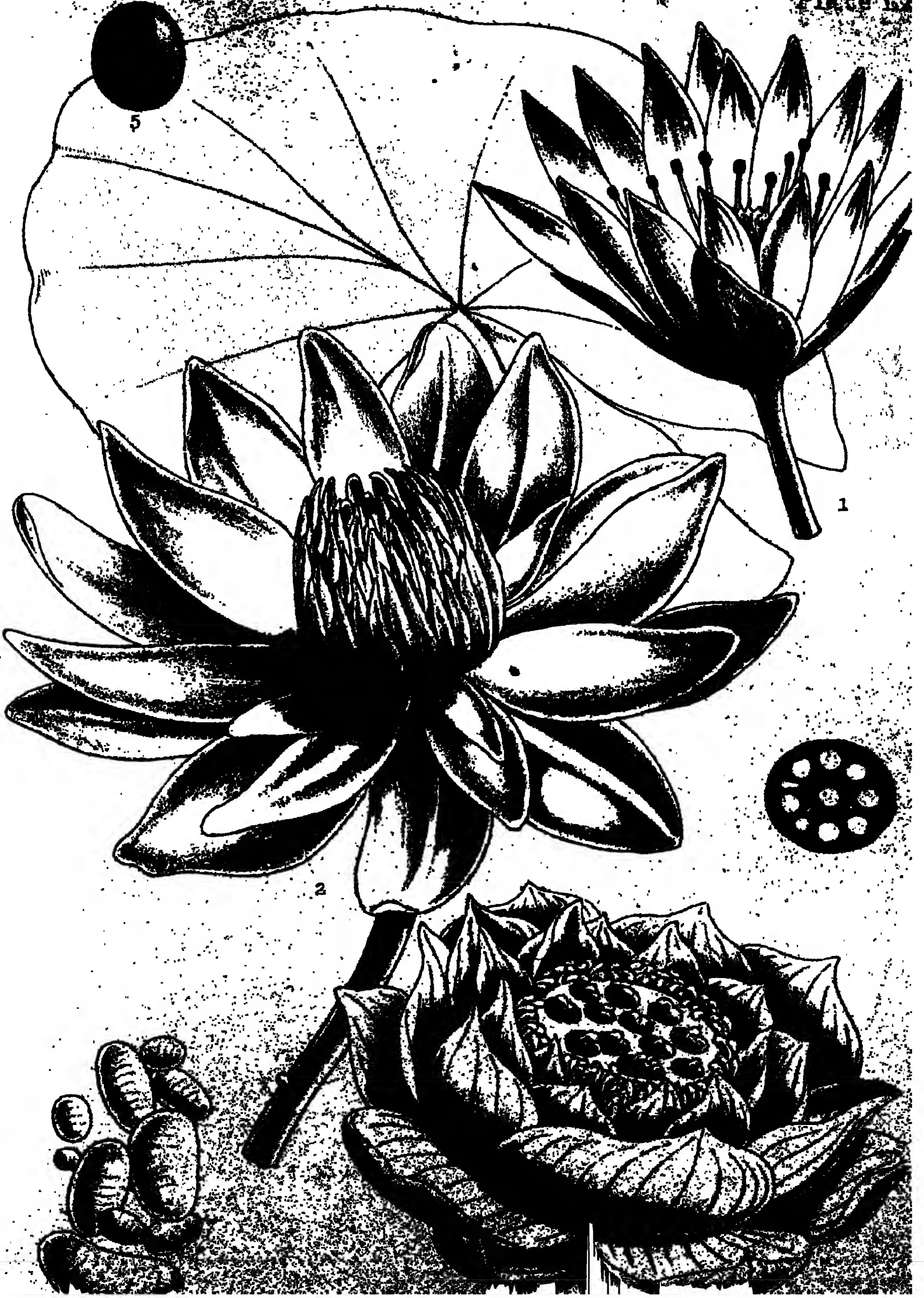
[PLATE LXXIV.]

THE history of sacred plants is always an interesting and instructive study; more so when it extends into a remote antiquity, and is associated with such great and advanced nations as those of Egypt and India. Much has been written and speculated concerning the Lotos of old authors; and great confusion has existed in many minds, on account of the desire to make all allusions and descriptions to harmonise with one ideal plant—the classic Lotos. At the outset of our remarks we must clearly intimate that it is impossible to combine all the fragments of history and description applied to some plant, or plants, known by the name of Lotos—and met with in the pages of Herodotus, Homer, Theophrastus, and others—into one harmonious whole, and apply them to a single mythical plant. It is manifest, from the authors themselves, that more than one Lotos is spoken of, and it was never intended to convey the notion that, like immortal Jove, the Lotos was one and indivisible. Starting, then, with the conviction that the one name has been applied to more than one or two very distinct and different plants, we shall have less difficulty than were we to attempt the futile task of reconciling all remarks about the Lotos to a single plant.

In the first instance, it is perfectly clear that the Lotos of Homer, which Ulysses discovered, and which is alluded to in the ninth book of the "Odyssey," is quite distinct from any of the rest. It is the fruit of this *tree* to which interest attaches, and not to the flower, as in some others. For the sake of distinction, we shall speak of this as the "arborescent Lotos," and attempt its identification.

The allusion to it by Homer will be more vividly present in the minds of readers than that of any other Lotos, since the story forms the basis of the "Lotos-eaters" of our own Tennyson. It is thus rendered by Pope :—

Plate



We touched, by various errors tossed,
The land of Lotos, and the flowery coast.
We climbed the beach, and springs of water found,
Then spread our hasty banquet on the ground.
Three men were sent, deputed from the crew,
(A herald one), the dubious coast to view,
And learn what habitants possessed the place.
They went, and found a hospitable race;
Not prone to ill, nor strange to foreign guest,
They eat, they drink, and Nature gives the feast;
The trees around them all their fruit produce;
Lotos the name; divine nectareous juice!
(Thence called Lotophagi) which whoso tastes,
Insatiate riots in the sweet repasts,
Nor other home, nor other care intends,
But quits his house, his country, and his friends:
The three we sent, from off the enchanting ground
We dragged reluctant, and by force we bound:
The rest in haste forsook the pleasing shore,
Or, the charm tasted, had returned no more.

This tree, the fruit of which was eaten by the Lotophagi, is mentioned by Herodotus, Theophrastus, Polybius, Dioscorides, and Pliny, and from them we gather the following particulars. "Of the Lotos, this particular kind is of a considerable size, about as large as a pear-tree, or somewhat less, having a leaf serrated like that of the *Quercus ilex*. The wood is of a dark colour" (Theophrastus). "The Lotus is a tree of no great height, rough and thorny, and bears a yellowish-green leaf, somewhat thicker and broader than that of the bramble" (Polybius). "The Lotos-tree is of a considerable size" (Dioscorides). "Of the size of a pear-tree, though Cornelius Nepos speaks of it as a shrub; the leaf is more serrated, otherwise it might be taken for the leaf of the evergreen oak" (Pliny). Virgil also includes it amongst trees. Hence we gather that it is a small, rough, thorny tree, with serrated leaves, and a dark wood, for Pliny adds, "the wood is of a black colour, and in request to make pipes to play upon." So much for the tree, and now as to its fruit. "This fruit of the Lotos is in size about as large as that of the *Lentiscus* (mastic); in sweetness it is like the fruit of the palm-tree. From this fruit the Lotophagi also make wine" (Herodotus). "The fruit is like the bean (Egyptian bean, or Kyamos); as the grape, it changes colour as it ripens; but, like myrtle-berries, it is produced thick and close upon the shoots. It is eaten by those people called Lotophagi; it is innocent, of an agreeable sweetness, and good for the bowels. There is one kind which has no stone, and that is sweeter; of this wine is made. The army

of Ophellas, on his march to Carthage, being short of provisions, is said to have subsisted for many days on this fruit" (Theophrastus). "Its fruit at first is like white myrtle-berries, both in size and colour, but when it ripens it turns to purple; it is then about the bigness of an olive; it is round, and, when ripe, has a small stone; it is gathered and bruised among bread-corn, put into a vessel, and kept as food for the servants; it is dressed after the same manner for the family, the kernel being first taken out; it has the taste of a fig, or date, but a far better scent. Wine is likewise made of it, by steeping, &c. Vinegar is also made of it" (Polybius). "It bears a sweet fruit larger than pepper" (Dioscorides). "Of this Lotos there are many varieties, and the varieties are most conspicuous in the fruit. This fruit is of the size of a bean, and of a saffron colour, but before it is ripe it undergoes many changes, like the grape. The fruit is produced in clusters, among branches, like myrtle-berries, and not as cherries are with us in Italy. The fruit affords so sweet a food that it has given name to a people and a district. It is said that those who eat of it are not subject to pains in the bowels. The better sort is without stone, for there is one kind that has a bony nut. From the fruit wine is also expressed" (Pliny). The whole of these agree in describing a sweet pulpy fruit, of variable size, but not larger than an olive, with a hard stone (and a stoneless variety, from which wine is made). There is no allusion whatever to any peculiar effects resulting from the eating of this fruit, of the kind indicated by Homer, so that this portion of his story may be eliminated as poetical. It may be a beautiful romance in the hands of our Laureate, to write of the "mild-eyed melancholy Lotos-eaters," and of the

> Branches they bore of that enchanted stem,
> Laden with flower and fruit, whereof they gave
> To each; but whoso did receive of them,
> And taste, to him the gushing of the wave
> Far far away did seem to mourn and rave
> On alien shores; and if his fellow spake,
> His voice was thin, as voices from the grave;
> And deep asleep he seemed, yet all awake,
> And music in his ears his beating heart did make.

We will accept the romance, and be thankful for the beautiful, even though it be romance, but the Lotos of the Lotophagi bore only a common-place sort of fruit which satisfied hunger, was sweet and pleasant, and could yield wine.

The next evidence to be adduced is that of modern travellers. First of these is Dr. Shaw, who states that the seedra of the Arabs "is a shrub very common in the Jereede, and other parts

of Barbary, and has the leaves, thorns, and fruit of the *Zizyphus* or jujube: only with this difference, that the fruit is there round, small, and more luscious, at the same time the branches are neither so much jointed nor crooked. This fruit is in great repute, it tastes something like gingerbread, and is sold in the markets all over the southern districts of these kingdoms. The Arabs call it 'Aneb enta el seedra,' that is, the jujube of the seedra."

That the arborescent Lotos was some species of *Zizyphus* or jujube, seems evident from the descriptions above given from ancient authors, and such is the impression of modern travellers. The observation of the late Dr. Lindley, to the effect that "the Lote bush, which gave its name to the ancient Lotophagi, is to this day collected for food by the Arabs of Barbary, who call it *Sadr*, and its berries *Nabk*, is the *Zizyphus Lotus* of botanists," is also the opinion of the majority of scientific men. It is possible that the fruit of other species of *Zizyphus* constituted some of the "varieties" mentioned by the ancients.

We must not omit to allude to the remarks made by Mungo Park, on this subject, in his "Travels." The negroes gathered from thorny bushes small fruits called *tomberongs*. These he describes as "small farinaceous berries, of a yellow colour, and delicious taste, which were no other than the fruit of *Rhamnus* (*Zizyphus*) *Lotus* of Linnæus. They had gathered two large baskets full in the course of the day. These berries are much esteemed by the natives, who convert them into a sort of bread, by exposing them for some days in the sun, and afterwards pounding them gently in a wooden mortar, until the farinaceous part of the berry is separated from the stone. This meal is then mixed with a little water, and formed into cakes, which, when dried in the sun, resemble in colour and flavour the sweetest gingerbread. The stones are afterwards put into a vessel of water and shaken about, so as to separate the meal which may still adhere to them; this communicates a sweet and agreeable taste to the water, and, with the addition of a little pounded millet, makes a pleasant gruel called *fondi*, which is the common breakfast in many parts of Ludamar, during the months of February and March. The fruit is collected by spreading a cloth upon the ground and beating the branches with a stick. The Lotus is very common in all the kingdoms which I visited, but is found in greatest plenty on the sandy soil of Kaarta Ludamar, and the northern parts of Bambarra, where it is one of the most common shrubs of the country. As this shrub is found in Tunis," he adds, "and also in the negro kingdoms, and as it furnishes the natives of the latter with a food resembling bread, and also with a sweet liquor, which is much relished by them, there can be little

doubt of its being the Lotos mentioned by Pliny as the food of the Libyan Lotophagi."

The characteristics of the jujube family are so much those described as pertaining to the classical Lotos, that no botanist appears to dissent from the conclusion that some species of *Zizyphus* is meant, and even at the time of Linnæus that particular species was indicated to which he applied the name of *Rhamnus Lotus*. It may be taken for granted, then, that during the intervening period, if this determination had been based upon insufficient grounds, or had a more promising candidate for the honour been discovered, some decided protest would have been entered against the pretender.

The application of the same trivial name to a species of *Diospyros* (the *D. Lotus*) cannot in itself be accepted as an assumption that it had claims to be regarded as the true Lotos in opposition to the jujube.

Whatever else the plant might be that is mentioned by Ovid as the Lotos, it was evidently arborescent. It occurs in the ninth book of his "Metamorphoses," where the nymph Lotis, fleeing from Priapus, is transformed into

> A flowery plant which still preserves her name;

and in the transformation of Dryope into such a tree for plucking the flowers of the "watery Lotos" for the amusement of her infant son, it is said that—

> The spring was new, and all the verdant boughs,
> Adorn'd with blossoms, promised fruits that vie
> In glowing colours with the Tyrian dye.

This may be the poetical romance of the origin of the Libyan Lotos.

The second Lotos may be designated as the Sacred Lotos, or Lotos of the Nile. It is the one which figures so conspicuously on the monuments, enters so largely into the decoration, and seems to have been interwoven with the religious faith of the Ancient Egyptians. This Lotos is mentioned by Herodotus, Theophrastus, Dioscorides, Pliny, and Athenæus as an herbaceous plant of aquatic habits, and from their combined description it seems evident that some kind of water-lily is intended. "When the river is full, and the plains are inundated, there grow in the water numbers of lilies which the Egyptians call Lotos" (Herodotus). "The Lotos, so called, grows chiefly in the plains when the country is inundated. The flower is white, the petals are narrow, as those of the lily, and numerous, as of a very double flower. When the sun sets they cover the seed-vessel, and as soon as the sun rises the flowers open, and appear above the water; and this is repeated, until the

seed-vessel is ripe and the petals fall off. It is said that in the Euphrates both the seed-vessel and the petals sink down into the water from the evening until midnight to a great depth, so that the hand cannot reach them; at daybreak they emerge, and as day comes on they rise above the water; at sunrise the flowers open, and when fully expanded they rise up still higher, and present the appearance of a very double flower" (Theophrastus). "The Lotos which grows in Egypt, in the water of the inundated plains, has a stem like that of the Egyptian bean (Kyamos). The flower is small and white like the lily, which is said to expand at sunrise, and to close at sunset. It is also said that the seed-vessel is then entirely hid in the water, and that at sunrise it emerges again" (Dioscorides). After describing the arborescent Lotos, Pliny states "there is also an herb of the same name, and in Egypt it grows up with an herbaceous stem, as a marsh plant. When the inundating waters of the Nile retire, it comes up with the stem like the Egyptian bean, with the petals crowded thick and close, only shorter and narrower. There is a further circumstance related concerning this plant of a very remarkable nature, that the poppy-like flowers close up with the setting sun, the petals entirely covering the seed-vessel; but at sunrise they open again, and so on, till they become ripe, and the blossom, which is white, falls off." Athenæus states that they grow in the lakes in the neighbourhood of Alexandria, and blossom in the heat of summer. He also mentions a rose-coloured and a blue variety. "I know that in that fine city they have a crown called Antinoëan, made of the plant which is there named Lotos, which plant grows in the lakes in the heat of summer, and there are two colours of it; one of them is the colour of a rose, of which the Antinoëan crown is made; the other is called Lotinos, and has a blue flower."

An aquatic plant, with double, poppy-like flowers, expanding in the morning and closing at night—

Those virgin lilies all the night
  Bathing their beauties in the lake,
That they may rise more fresh and bright
  When their belovèd sun's awake—

either white, blue, or rose-coloured, points clearly to a nymphæaceous plant, closely allied to our own white water-lily; and this is further strengthened by the description of the fruit.

"These (Lotos) they gather and dry in the sun; then they pound what is obtained from the middle of the flower, which is like a poppy-head, and make it into leaves, which they bake" (Herodotus). "The size of the seed-vessel is equal to that of

the largest poppy-head, and it is divided by separations, in the same manner as the seed-vessel of the poppy; but the seed, which is like millet,* is more condensed. The Egyptians lay these seed-vessels in heaps to perish, and when they are rotten the mass is washed in the river, and the seed taken out and dried, which is afterwards made into loaves, baked, and used for food" (Theophrastus). "The seed-vessel is like a very large poppy-head, and the seeds are like millets, which the Egyptians dry and make into bread" (Dioscorides). "It has a seed-vessel in all respects like a poppy-head, and contains seeds like millet. The inhabitants lay these seed-vessels in heaps to putrefy, then wash away the filth, dry the seed, pound it, and make bread of it" (Pliny).

Anyone acquainted with our native water-lilies will recognise this description of the poppy-like fruit and numerous seeds. Those who are not may consult the 53rd plate of the third edition of Sowerby's "English Botany." This is still more corroborated by the remark of Theophrastus, that "the nature of the stem is like that of the bean (Kyamos), and its spreading leaves are similar, except that they are less and thinner, and the leaf is attached to its foot-stalk in the same manner." The nature of the stem and petioles, alluded to in the bean or Kyamos, will be described hereafter, and consists of internal cavities, or air-passages, to be seen also in the sections of the white water-lily in the plate of Sowerby's "Botany" already quoted. (Pl. LXXIV., fig. 4.)

There is one other part of the plant alluded to by the ancients which must not be omitted—the farinaceous root. "The root also of the Lotos is eatable, and moderately sweet; it is round, and of the size of an apple" (Herodotus). "The root of the Lotos is called *corsion*, which in its figure and size is like a quince: the colour of the rind is dark, like a chestnut, but the inside is white; when boiled or baked it is like the yolk of an egg (this expression is doubtful as to its meaning), and agreeable to the taste; it is also eaten raw" (Theophrastus). "The root, which in appearance is like a quince, is eaten both raw and boiled; when boiled, in quality it is like the yolk of an egg" (Dioscorides).

From these descriptions it is evident that the Sacred Lotos of the Nile, the Egyptian Lotos of the ancients, was a species of *Nymphæa*, common in the waters of that river. Plants, and animals also, submit so much to external circumstances, that the lapse of centuries may eradicate them from spots on which

* The common millet of Egypt was the Dhoora, or *Sorghum vulgare*, the seeds of which are much larger than in the millets belonging to the genera *Setaria*, *Panicum*, or *Paspalum*.

they were at one time common. It by no means follows that the same plants will be found flourishing in the Nile now, that were common under the Pharaohs; but, when the French invaded Egypt in 1798, Savigny brought home from the Delta a blue *Nymphæa*, which was figured in the "Annales du Museum," corresponding very closely in habit to the conventional Lotos so common on the Egyptian monuments.

It seems to be very probable that the Lotos-flower in the hands of the guests at Egyptian banquets, and those presented as offerings to the deities, were fragrant. The manner in which they are held strengthens this probability, as there is no other reason why they should be brought into such close proximity with the nose. Our figure of a flower of the blue *Nymphæa* (*N. cærulea*), from Savigny, should be compared in habit, narrow acute petals, &c., with the Lotos of the monuments. (Pl. LXXIV., fig. 1).

The white Lotos is evidently *Nymphæa Lotus*, L., which is common to the floras of India and Egypt. Like others of the order, it has a tendency to variation to such an extent that numbers of its forms have received specific names; but these are united by Dr. Hooker under the above name. The red Lotus is a coloured variety of the same plant. Andrews recognised it as a distinct species allied to *N. Lotus*, but specifically distinct in the colour of the flowers. Roxburgh declares that the difference between them is in the colour of the flowers only. It is now generally admitted that the colour of the flowers is insufficient, of itself, to constitute a specific difference, and the red Lotus, or *Nymphæa rubra*, becomes merely the red-flowered *Nymphæa Lotus*, L. (Pl. LXXIV., fig. 2.)

As to the blue Lotus, it is our opinion that it must be referred to *Nymphæa stellata*, Willd., as accepted by Hooker and Thomson in "Flora Indica." Of this species the *N. cyanea* of Roxburgh and the *N. cærulea* of Savigny are varieties. It is stated in the work just cited that the authors had long entertained the belief that "the blue water-lily of the Nile and India are (like their white congener *Lotus*) specifically the same. The most prominent difference we find between them is the sweet scent of the African plant, whether wild or cultivated, and its usually more numerous petals and stamina." It is the fragrant blue Lotus of the Nile that seems to be represented so freely on the monuments, whether it be called *Nymphæa cærulea* or *N. stellata*. And it is the white Lotus (*Nymphæa Lotus*) alluded to by the majority of ancient authors. The seeds and roots of both species are commonly eaten by the natives of India.

There is still a third Lotus, briefly mentioned by Dioscorides, Theocritus, and Homer, which may be some species of *Medi-*

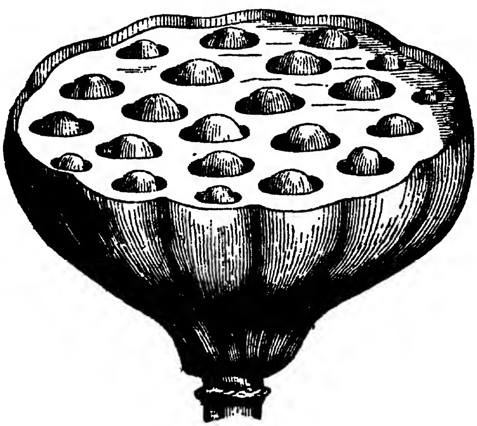
*cago* or of the modern genus *Lotus*. It is herbaceous, sometimes wild, and sometimes cultivated; but always written about as though constituting herbage, and is on one occasion cropt by the horses of Achilles. We shall not pause to identify this plant, but proceed at once to the last plant it is our design to deal with.

The Kyamos, or Indian Lotos. This can scarcely claim to be one of the kinds of Lotos mentioned by the ancients, since it is distinctly alluded to by them as the Egyptian bean, or Kyamos. This plant amongst the Hindoos has a sacred character, equal to that of the Lotos amongst the Egyptians. It was doubtless Asiatic in its origin, but at one time was plentiful in Egypt, whence it has now totally vanished. It is represented on the Egyptian monuments, but far less commonly than the Sacred Lotos. Some authors declare this to be the veritable "Sacred Lotos of Egypt," a title to which it has no claim. Herodotus, after describing the Lotos, adds, "there are likewise other lilies, like roses (and these, too, grow in the river Nile), whose fructification is produced in a separate seed-vessel, springing like a sucker from the root, in appearance exactly resembling a wasp's nest, and containing a number of esculent seeds, about the size of olive-berries. These are also eaten, when tender and dry."

The descriptions of this plant are so characteristic that there never appears to have been any doubt as to what plant was intended. "This Egyptian bean," says Dioscorides, was "chiefly produced in Egypt, and in Asia, and in Cilicia, in stagnant waters." He says it has a large leaf like an umbrella, and a stem a cubit high, of the thickness of a finger. The flower, which is like a rose, is twice the size of a poppy: when the petals fall off, the seed-vessel is produced with cells, each containing a bean, a little elevated above the top of the seed-vessel, like a bubble in water. The seed-vessel is called ciborion or cibotion, and the planting of the beans is effected by sinking the seed-vessel in the water, with the beans in it, so that they may take root in the mud. The root is thicker than a reed, and it is eaten both boiled and raw, and is called collocasia; the bean is also eaten green: when it is dry it becomes of a dark colour, and is larger than the Grecian bean.

Theophrastus has also given rather a full description of this plant. He says that "it is produced in marshes and in stagnant waters; the length of the stem, at the longest, four cubits, and the thickness of a finger, like the smooth jointless reed. The inner texture of the stem is perforated throughout like a honeycomb, and upon the top of it is a poppy-like seed-vessel, in circumference and appearance like a wasp's nest. In each of the cells there is a bean projecting a little above the surface

of the seed-vessel, which usually contains about thirty of these beans or seeds. The flower is twice the size of a poppy, of the colour of a full-blown rose, and elevated above the water; about each flower are produced large leaves, of the size of a Thessalian hat, having the same kind of stem as the flower-stem. In each bean, when broken, may be seen the embryo plant, out of which the leaf grows. So much for the fruit. The root is thicker than the thickest reed, and cellular like the stem; and those who live about the marshes eat it as food, either raw, or boiled, or roasted. These plants are produced spontaneously, but they are cultivated in beds. To make these bean-beds, the beans are sown in the mud, being previously mixed up carefully with chaff, so that they may remain without injury till they take root, after which the plant is safe. The root is strong, and not unlike that of the reed; the stem is also



Receptacle of *Nelumbium speciosum*, with the seeds *in situ*.

similar, except that it is full of prickles, and therefore the crocodiles, which do not see very well, avoid the plant, for fear of running the prickles into their eyes."

Major Drury observes that the mode of sowing the seeds of *Nelumbium* in India, at the present day, is by first enclosing them in balls of clay, and then throwing them into the water.

Sir James Smith says that in process of time the receptacle separates from the stalk, and, laden with ripe oval nuts, floats down the water. The nuts vegetating, it becomes a cornucopœia of young sprouting plants, which at length break loose from their confinement, and take root in the mud.

The account given by Strabo of the Egyptian bean is not less interesting. "In the marshes and lakes of Egypt grow both the paper-reed and the Egyptian bean, which produces a

*ciborium*. They are nearly of equal height, having stems about ten feet long; but the paper-reed has a smooth stem, with foliage growing from the top, whereas the bean has leaves and flowers springing from separate stems, and bears a fruit like our bean, but differing in size and taste. The plantations of beans are pleasant to the sight, and delightful to those who wish to feast on them. The way of feasting is to go in boats with cabins into the thick plantations of them, where a shade is afforded by the leaves, which are very large, so as to be used for cups and bowls: they are adapted to this use by their concavity. The shops at Alexandria are full of them, where they are used for vessels. The sale of them constitutes one part of the profit of a farm."

Comparing all these very characteristic features with what is known of *Nelumbium speciosum*, there is no room for doubt that this is the plant which was known to the ancients as the Kyamos or Egyptian bean, the Tamara of modern India. This is described as an aquatic plant, with orbicular leaves attached at the centre, smooth; under-surface pale; margins somewhat waved; peduncles longer than the petioles, and erect; root-stock horizontal, fleshy, sending out many fibres from the under-surface; petioles long, rising above the surface of the water, rough with acute tubercles; corolla polypetalous; flowers large, white or rose-coloured; nuts loose in the hollows of the torus or receptacle. (Pl. LXXIV., fig. 3.)

The flowers-stems and petioles of the leaves are pierced throughout their length with numerous canals or air-ducts, so that sections cut from them are full of large holes. These sections, about one-eighth of an inch in thickness when dried, are sold in the bazaars of India, where they are employed medicinally. (Pl. LXXIV., fig. 4.)

The leaves and flower-stalks abound in spiral tubes, which are extracted with great care by gently breaking the stems and drawing apart the ends; with these filaments are prepared those wicks which are burnt by the Hindoos in the lamps placed before the shrines of their gods. In India, as well as in China and Ceylon, the flowers are held to be especially sacred. The roots and seeds are still eaten in India, as they were in ancient Egypt; and Dr. Porter Smith states that the common arrow-root of China is prepared from this plant. (Pl. LXXIV., fig. 6.) The peculiar receptacle, and the way in which the seeds are immersed in it, are so characteristic, that it needs scarcely any other evidence to establish the identity of the *Nelumbium* with the Kyamos.

The Chinese distinguish four kinds of water-lily—the yellow, the white, the red, and the pink; the three latter sometimes with single flowers, sometimes with double. Of the *Nelumbium*

M. Huc says:* "This plant may be propagated by seeds, but more easily and rapidly by roots; it does not require any kind of culture, and there is nothing comparable to the effect produced by this splendid flower on the ponds and basins of China. It does not bud till towards the end of May, but its germination is very rapid, and its great leaves, lying on the surface of the water or raised majestically to various heights, form a covering of most exquisite verdure, the beauty of which is of course enhanced when it is enamelled by flowers of various dyes. They are larger than poppies, and their dazzling tints are beautifully relieved by the green leaves. The young Chinese poets are particularly fond of celebrating the beauty of the water-lily gleaming in the moonlight, as the boats row about the basins illumined by swarms of glow-worms and fire-flies. Its seeds are eaten as nuts are in Europe (fig. 5), and boiled in sugar and water they are considered delicious by epicures. The gigantic root is a great resource for culinary preparations, and in whatever way it is dressed it is always excellent and wholesome. The Chinese pickle great quantities of it with salt and vinegar, to eat with rice; reduced to powder, it is extremely agreeable when boiled with milk or water, and in the summer it is eaten raw like fruit, and is very refreshing. Finally, the leaves are constantly made use of instead of paper for wrapping up all kinds of things, and when dried are often mixed with tobacco, to render it a little milder."

Although we cannot regard this as the mythic Lotos of Egypt, it was doubtless held in veneration, and is, moreover, considered sacred by the Hindoos, and serves for the floating shell of Vishnu and the seat of Brahma. Sir William Jones says that "the Thibetans embellish their temples and altars with it, and a native of Nepaul made prostration before it on entering my study, where the fine plant and beautiful flowers lay for examination." Thunberg affirms that the Japanese regard the plant as pleasing to the gods, the images of their idols being often represented sitting on its large leaves. In China the Shing-moo, or Holy Mother, is generally represented with a flower of it in her hand, and few temples are without some representation of the plant.

According to Chinese mythology, Shing-moo bore a son, while she was a virgin, by eating the seeds of this plant, which lay upon her clothes on the bank of a river where she was bathing. When the time of her gestation was expired, she returned to the same place, and was there delivered of a boy. The infant was afterwards found and educated by a poor fisherman, and in process of time became a great man and performed

* "The Chinese Empire," by M. Huc. London, 1859, pp. 469, 470.

miracles. When Shing-moo is represented standing, she generally holds a flower of the *Nelumbium* in her hand; and when sitting, she is usually placed upon one of its large orbicular leaves.

The conclusion to which these observations are directed is, that at least four kinds of Lotos are mentioned by the ancients. That one of these is arborescent, and bore the fruit on which the Lotophagi subsisted, which was some one or more species of *Zizyphus*, chiefly *Zizyphus Lotus*. That a second of these was the Sacred Lotos of the Nile, a water-lily or *Nymphæa*. That the third was an herbaceous or leguminous pasture plant. Finally, that, although the Egyptian bean, or Kyamos, was known to the ancients, is represented on the monuments of Egypt, and was probably held in veneration, it was not alluded to by ancient authors as a Lotos, and undoubtedly was the *Nelumbium speciosum* of botanists.

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## EXPLANATION OF PLATE LXXIV.

FIG. 1. Flower of blue water lily of the Delta of Egypt (*Nymphæa cærulea*), from Savigny.
" 2. Flower of white water lily of the East (*Nymphæa Lotus*).
" 3. Flower of Kyamos, or "Egyptian bean" (*Nelumbium speciosum*).
" 4. Section of flower-stem of same plant.
" 5. One of the nuts, or seeds, of *Nelumbium speciosum*, natural size.
" 6. Starch from the root-stock of *Nelumbium speciosum*, x. 320.

# GREENLAND.

By WILLIAM PENGELLY, F.R.S., F.G.S.

GEOGRAPHERS and geologists have for some time devoted a large amount of attention and labour to Greenland; and, judging from the numerous reports and papers on it, which have been read to various scientific bodies, it must be admitted that their efforts have been crowned with great success. Of these communications, those which have most recently arrested our attention are Professor Heer's "Contributions to the Fossil Flora of North Greenland," read to the Royal Society of London on March 11, 1869,* and Dr. Brown's "Physics of Arctic Ice," read to the Geological Society of London on June 22, 1870,† the latter treating of the country as it is at present, and the former of its condition during the Miocene period of the geologist.

From time to time, Arctic voyagers—especially McClintock, Inglefield, Colomb, and Olrick—have brought from Greenland considerable collections of fossil plants, which have been lodged in the museums of London, Dublin, and Copenhagen. They have attracted so much attention, and their revelations have been so startling, as to induce the Royal Society of London and the British Association to vote, in 1866, liberal grants of money for the purpose of investigating the fossiliferous beds, and making as complete a collection as possible of the remains of the plants which they contain. The expedition was entrusted to Mr. E. Whymper, so well known for his Alpine researches, and Dr. Brown, who had previously travelled in Arctic North America, Greenland, and Spitzbergen, and had availed himself of the ample opportunities he had thus enjoyed for studying ice phenomena.

They reached the colony of Jacobshavn, in Greenland, on June 16, 1867, and left the island on the 10th of the following September, having received, during their stay, every assistance

* See "Phil. Trans." for 1869, Pt. II. pp. 445–488.
† See "Quart. Journ. Geol. Soc." vol. xxvi. pp. 671–701.

from the Danish authorities. The fossils they brought home were submitted to Professor Heer, the eminent botanist of Zurich, whose report on them has already been named.

Greenland is in all likelihood a large wedge-shaped island, covered everywhere in the interior with a sheet of ice of unknown depth. The coast-line surrounding this vast *mer de glace* is of variable breadth, and has the aspect of a circlet of bare bleak islands rising to the height of about two thousand feet, and separated by deep inlets or fjords, which are the channels through which the overflow of the interior ice finds its way to the sea. During the short Arctic summer the snow clears off this outskirting land, on which the population of Greenland lives and the Danish trading-ports are built.

Though a familiar subject of conversation among the colonists from the earliest times, very few of them have ever visited the great interior sea of ice; whilst the natives have a great horror of it, not only because of the dangers it presents, but from a belief that it is inhabited by evil spirits of monstrous forms. At the inlets, where the interior ice sometimes reaches the sea, it presents 'ice-walls,' varying in height from one thousand to three thousand feet, according to the depth of the valley. This wall is always steep, because bergs are continually breaking off from it, thus rendering approach to it very dangerous, on account, not only of the falling ice, but of the waves which it produces. One of these faces, known as Humboldt's Glacier, is about sixty miles broad.

Once fairly on the ice in the interior, a dreary scene meets the view—one great ice-field, unbroken in all directions, except in those in which the outskirting land is seen. The traveller, however, finds it traversed with *crevasses*, the bottom of which he is unable to see, or to reach with his sounding-line. The surface of the field rises continuously but gently, the gradient diminishing towards the interior. In the winter it must be covered with a deep layer of snow, and the surface must be smooth as a glassy lake; but in the summer this covering is converted into water, which, in the form of streams, finds its way to the sea, directly by flowing on the surface to the edge, or indirectly by falling into the *crevasses*, and thence by subglacial routes. As is the case with glaciers generally, the surface of the ice is ridged and furrowed; and so far as observations have gone, this increases towards the interior. Nowhere is there to be seen on it a trace of any living thing, or a patch of earth, or a stone, or, in short, anything whatever to remind one of the outer world. An afternoon breeze blows over it regularly with such piercing bitterness, that the explorers found their Eskimo dogs crouched under the lee of the sledge for shelter.

There seems every probability that the country is covered with one continuous almost level field of ice, concealing or obliterating all indications of hill and valley, without a single break, for upwards of twelve hundred miles from north to south, and four hundred from east to west. Its thickness is unknown; but when it is remembered that every square mile contains six hundred and forty acres, that the weight of an inch of rain is upwards of one hundred tons per acre, and that, even exclusive of the pressure, the specific gravity of ice is about eight-ninths of that of water, it will be seen that the unbroken ice-field of Greenland must have an area of upwards of three hundred million acres, and a weight of more than twenty-seven thousand million tons for every inch of its thickness.

From the facts that ice-bergs are rare on the east coast, and that no stones or other indications of land are found on the surface of the ice-field, it is thought probable that there is no high land in the interior, but that the ice slopes continuously from east to west; and as the surface of the vast accumulation of ice in the known interior, so far from anywhere attaining the height of the circumscribing land, can only be seen by climbing to considerable elevations on the latter, it is believed by Dr. Brown that the bare surface of the country, were its glacial covering removed, would resemble a huge shallow vessel with high walls around it—a vessel now filled with ice, which slowly flows off, in the form of glaciers, through the enormous lips in the zone of mountain-land forming its rim. Dr. Brown is of opinion that a great inlet once stretched across the island from Jakobshavn ice-fjord, as represented on the old maps, but that it is now choked up with consolidated bergs.

It can scarcely be doubted that, in the course of ages, the glaciers, slowly travelling seaward, grind down the bottoms of the valleys to the sea-level, and thus convert the valleys themselves into fjords, such as are so prevalent on the coasts of northern countries in general. When a glacier reaches the sea, it grooves its way along the submarine bottom for a considerable distance—in some instances upwards of a mile—until it is stopped by the buoying action of the water, through which, and not the force of gravity, a portion is ultimately broken off and an ice-berg is formed. "The ice," says Dr. Brown, "groans and creaks, then there is a crashing, then a roar like the discharge of a park of artillery, and with a monstrous regurgitation of waves, felt far from the scene of disturbance, the ice-berg is launched into life." Some of the bergs may be seen sailing majestically in long lines out of the ice-fjords, to be wafted in various directions by the winds and currents. Some of them ground near the fjords, where they remain for months

or even years, and are only removed by "calving," or pieces breaking off from them.

Dr. H. Rink, of Copenhagen, whose long residence in the country entitles his opinion to the greatest respect, has calculated the yearly precipitation, including both snow and rain, at ten inches, and the discharge of ice, in the form of glaciers, at two inches. A small portion is given off by evaporation, but the greatest discharge is probably in the streams of water which pour out beneath the glaciers, both in summer and winter. We do not appear to be in possession of sufficient data to justify an opinion as to how far the united yearly discharge of ice, water, and vapour at present equals the annual precipitation. It is obvious that the question of the increase or decrease of the existing ice-sheet hinges on this point.

The sub-glacial streams, thickly loaded with mud from the grinding of the glaciers on the rocks over which they travel, discolour the sea for miles, and finally deposit on the bottom a thick coating of the finest material, in which Arctic marine animals burrow in great numbers. Some of the inlets, formerly quite open for boats, are now so choked up with bergs —mainly, it is thought, in consequence of the deposits of sub-glacial mud—that going up them is never thought of at present.

Occasionally, without a breath of wind stirring, ice-bergs are seen "shooting out" of an inlet, propelled, in all probability, by the waves produced by a fresh berg being detached from the glacier up the fjord.

The bergs when aground have always a slight movement, which stirs up the food on which the seals largely subsist; hence the neighbourhood of such bergs is a favourite haunt of these animals, and thus too often tempts the native fisherman, who not unfrequently loses his life by falling ice. "When we would row between two bergs," says Dr. Brown, "to avoid a few hundred yards' circuit, the rowers would pull with muffled oars and bated breath. Orders would be given in whispers, and even were Sabine's gull or the great auk to swim past, I scarcely think that even the chance of gaining such a prize would tempt us to run the risk of firing, and thereby endangering our lives by the reverberations bringing down pieces of crumbling ice hanging overhead. A few strokes and we are out of danger; and then the pent-up feelings of our stolid fur-clad oarsmen find vent in lusty huzzahs! Yet, when viewed out of danger, this noble assemblage of ice palaces, hundreds in number being seen at such times from the end of Jakobshavn Kirke, was a magnificent sight; and the voyager might well indulge in some poetic frenzy at the view. The noonday heat had melted their sides; and the rays of the red even-

ing sun glancing askance among them would conjure up fairy visions of castles of silver and cathedrals of gold. . . Suddenly there is a swaying, a moving of the water, and our fairy palace falls in pieces, or, with an echo like a prolonged thunder-clap, it capsizes, sending the waves in breakers up to our very feet."

Ordinary Alpine glaciers, like those of Switzerland, flowing down mountain gorges, receive great accumulations of rocky débris on each side, which are termed *lateral moraines.* In the frequent case of two such gorges uniting in one at a lower level, what may be called the *adjacent* or *inner* laterals become one, and form a *medial moraine.* Not unfrequently portions of the material thus accumulated on the surface fall through the *crevasses*, and, reaching the bottom, participate there in the general downward motion, and with the débris the glacier has dislodged from the rocky surface on which it travels, form the *moraine profonde* or *basal moraine.* If, as in the Alps, the glacier terminates without reaching the sea, most of the matter thus transported is deposited at its foot, and forms a *terminal moraine.*

The glaciers of Greenland are much more simple. They bring no débris from the interior; and the short valleys through which they reach the sea rarely unite. The surface material—which is inconsiderable, and seldom takes the form of a medial moraine—together with that at the base, is floated off by the detached bergs, which not unfrequently capsize in the inlets, and thus deposit, at least, the greater part of their burthen before reaching the open sea. Hence, could the submarine surface be inspected, it would in all probability be found to consist of tenacious clay, imbedding a long line of boulders, shells, and bones of seals and other marine animals. This matter must frequently be re-arranged by the enormous momentum of ice-bergs grounding on it. Dr. Brown mentions the case of a berg which, in 1867, he observed at the mouth of the Waygatz, carrying a block of rock that, even at a distance, looked as large as a good-sized house.

Greenland, though so intensely cold, and apparently so cheerless, is full of interest to the naturalist, and by no means without profit for the merchant. The outskirting land supports a luxuriant growth of from 300 to 400 species of plants, some of which ascend to the height of 4,000 feet; many species of seals, and whales, and fish sport in the waters, which are also occupied by invertebrate animals and seaweeds; every rock swarms with water-fowl, whilst land-birds from the south visit the country as a nesting-place; countless herds of reindeer browse in some of its valleys; the bark of the fox is to be heard even in the depth of winter; and the polar

bear may be seen all the year round. The Danes, at their first visit, found a human population there of 30,000; and within their own possessions there is at present a healthy, intelligent, civilised race of hunters of not less than 10,000 souls. Exclusive of home consumption, the annual exports of the settlements amounted in 1835 to 9,569 barrels of seal-oil, 47,809 seal skins, 1,714 fox skins, 34 bear skins, 194 dog skins, 3,437 lbs. of eider down, 5,206 lbs. of feathers, 439 lbs. of narwhal ivory, 51 lbs. of walrus ivory, and 3,596 lbs. of whalebone.

Geologists have long taught that, at least, the west coast of Greenland is slowly sinking below the sea. This doctrine is confirmed by Dr. Brown, who recapitulates the principal points of the evidence on which it rests. The following are amongst the facts he enumerates:—Near the end of the last century a small rocky island was observed to be entirely submerged at springtide high-water, yet on it were the remains of a house, rising six feet above the ground; fifty years later the submergence had so far increased that the ruins alone were *ever* left above water. The foundations of an old storehouse, built on an island in 1776, are now dry only at low water. The remains of native houses are in one locality seen beneath the sea. In 1758 the Moravian Mission establishment was founded about two miles from Fiskernæsset, but in thirty years they were obliged to move, at least once, the posts on which they rested their large *omiaks*, or seal-skin boats. Some of the posts may yet be seen under water. The dwellings of several Greenland families, who lived on Savage Point from 1721 to 1736, are now overflowed by every tide. In one locality, the ruins of old Greenland houses are only to be seen at low water. A blubber house, originally built on a rocky islet about a furlong from the shore in Disco Bay, had to be removed in 1867, as the floor was flooded at every tide, in consequence of the gradual sinking of the islet—a fact which had long been recognised. An adjacent island, on which the natives formerly encamped in considerable numbers during summer, has become so diminished in size through slow subsidence that there is at present room for no more than three or four skin tents. Dr. Brown estimates the rate of submergence at not more than five feet in a century.

Proofs of an *upward* movement appear to be equally well established on the north coast, where Dr. Kane, in 1855, observed and described a series of old sea-beaches rising one over another to considerable heights above the sea-level. "I have studies," he says, "of these terraced beaches at various points on the northern coast of Greenland. . . . As these strange structures wound in long spirals around the headlands of the

fjords, they reminded me of the parallel roads of Glen Roy—a comparison which I make rather from general resemblance than ascertained analogies of causes." *

There seems a tendency to regard this upward movement in the north, as well as the downward movement in the west, as still in progress; in fact, to consider Greenland as a sort of lever, having its fulcrum somewhere between the two regions in which the opposite changes of relative level have been observed. There is nothing inconsistent in the hypothesis that a subsidence in one region synchronises with elevation in another at no very great distance; and, indeed, it is believed by, at least, most geologists that an instance of the kind is furnished by Sweden, which is rising along the coast of the Gulf of Bothnia, sinking in the extreme south of the peninsula, but undergoing no change in the district of which Stockholm may be regarded as the centre. Dr. Brown, however, whilst cordially accepting the evidence of upheaval in North Greenland, believes that movement to be a thing of the past, that the whole island participated in it, and that he has detected unmistakeable proofs, along the whole extent of the Danish colonies—and, in one instance, 500 feet above the sea—of a striated clay, containing shells belonging to species still living in the neighbouring sea. In like manner, he regards the subsidence now in progress as being by no means local, but shared by the entire country. He admits, however, that the district between the Danish settlements and the south coast have not been examined; so that he can only be held to have proved that, since the advent of the species of shellfish now living in the adjacent sea, those parts of Greenland now known to be sinking were at a much lower level than they are at present; that, even then, the country was the scene of ice action, which, by depositing glacier-clay, furnished a habitat for the marine mollusks whose shells are now found in it; that after this deposition the district rose slowly above the sea, and attained a sub-ærial height of many hundred feet; that if the process of elevation resembled that in the north of the island, it was broken by protracted periods of intermittence, during which the successive terraces were formed; and that, at length, there set in a movement in the contrary direction, which is still in progress. It does not appear from the evidence at present before us that the downward movement is necessarily shared by the north, or, indeed, that the elevation has yet ceased there. On these points we need further information.

It is obvious that whilst the changes just described take us slowly and far back into antiquity, they fail to reach the com-

* "Arctic Explorations,' vol. ii. p. 81.

mencement of the glacial condition of the country. The clays, which, notwithstanding the present slow subsidence, are still 500 feet *above* the sea-level, were due to *glacial* agency, and must have been deposited when the areas in which they occur were far *below* the sea. They are occupied, too, by shells of the same species as now live in Greenland waters, and thus denote that the climate has not changed.

The existing ice-sheet, which so completely covers the land—concealing alike the tops of the mountains and the valleys which separate them—is eloquent of time. It represents, not the accumulated total snows of ages, but the sum of the annual surpluses—the remnants of the yearly precipitation which the conjoined actions of evaporation, ice-flow, and sub-glacial streams have failed to remove—the hoarded capital resulting from the excess of ice-income over expenditure in every form. And yet this income is estimated at no more than ten inches annually, so that the yearly savings must have been very inconsiderable in themselves—probably an inch or two, at most. Their aggregate is vast, merely because the time of accumulation has been very protracted.

It is obvious that the geologist's chance of finding fossils is limited to the outskirting land. Here, however, and especially near Atanekerdluk, on the western coast, opposite Disco Island, in latitude 70° N.—termed North Greenland by Dr. Heer—he has been eminently successful, as has been already remarked.

From the Report of Professor Heer, it appears that the specimens collected by Mr. Whymper and Dr. Brown contained 89 species of plants, of which 20 were entirely new to science; that we are now acquainted with a total of 137 species from the same beds and localities; and that the deposits which yielded them belong to what is known to the geologist as the Miocene age—a period very remotely ancient, no doubt, when measured by even the largest unit employed in human history, but not very far back in the vast antiquity of the world. It was separated from the close of that era in which our chalk beds were formed, by a period termed the Eocene, and, in all probability, by an earlier but unrepresented interval. It was long prior, on the other hand, to the first appearance in the world of any existing species of quadrupeds, and though some of the kinds of shell-fish now living were also living then, upwards of fifty per cent. of the species forming the present molluscous fauna date from times less ancient than those represented by the plant-beds of Atanekerdluk.

Plants of the same kind and of the same age have been found also in Iceland, and even in Spitzbergen in latitude 78° 56′ N., and are wonderfully calculated to revolutionise our notions of the climate of the Arctic regions. That it cannot always have

been frigid, is evident from the facts that of the fossils in question considerably more than half the number were trees, whilst at present no trees exist in any part of Greenland, though its southern point, Cape Farewell, is in latitude 59° 47′ N., or fully 700 miles farther south than Atanekerdluk; that amongst them there were upwards of thirty different kinds of cone-bearing trees, including several species allied to the gigantic Wellingtonia at present growing in California; that the other trees were beeches, oaks, planes, poplars, maples, walnuts, limes, a magnolia, hazel, blackthorn, holly, logwood, and hawthorn; that they were not represented by leaves merely—which occurred, however, in vast profusion—but by fossil flowers and fruits, including even two cones of the magnolia, thus proving that they did not maintain a precarious existence, but ripened their fruits. Ivies and vines twined round their trunks, beneath them grew ferns having broad fronds, and with them were mingled several evergreen shrubs.

They were by no means confined to high latitudes, for at least forty-six of the species have been found as fossils in Central Europe. So far as is at present known, six of them grew no farther south than the Baltic, ten have been found in Switzerland, seven in Austria, four in France, seventeen in Italy, six in Greece, and four in Devonshire. In fact, these extinct old Miocene plants had a much wider geographical range than is enjoyed by their allies in the present day; whence Professor Heer has concluded that the temperature of the northern hemisphere, at least from Greece to within a few degrees of the Pole, was much more uniform during the Miocene era than it is at present. The mean annual temperature of North Greenland was, he believes, 30°, and of Central Europe 10°, higher than it is now.

A vegetation so luxuriant was probably the home of a large and varied amount of animal life; though, up to this time, their remains have been but very sparingly found. Professor Heer, however, has detected two fossil insects—one of them a beetle—amongst the leaves.

Such, it has been well remarked, was the variety, luxuriance, and abundance of this old Miocene flora, that if land extended at that time from Greenland to the Pole, it was probably occupied by at least many of the same species of plants.

# OBSERVATIONS ON JUPITER IN 1870–71.

By the Rev. T. W. WEBB, M.A., F.R.A.S.

THE phenomena of Jupiter during and after the opposition in 1869 formed the subject of a detailed examination, the principal features of which appeared in the POPULAR SCIENCE REVIEW for April, 1870. Although no very important conclusion was deduced at that time, it seemed desirable that the observations should be continued during another opposition, as the extension of a series always possesses a certain value, whether it may be for confirmation or correction, and either change or persistency would not be without its own interest or significance. Accordingly, on Nov. 15, 1870, the planet was again brought into the same telescopic field, and the results of a scrutiny carried on through forty-nine nights, till April 15, 1871, are now laid before the public. The nomenclature originally adopted having still answered satisfactorily the purpose of identification, we shall speak, as before, of the brighter parts of the disc as the *Equatoreal* and *Two Temperate Zones*, and of the darker stripes as the *Two Torrid* and *Two Temperate Belts*, to which must be added the *South Sub-torrid Belt*, subdividing the South temperate zone, and the *Two Polar Regions*.

A comparison of the former observations with those of Mr. Gledhill, at Mr. Crossley's observatory, Halifax, and those of Professor Mayer, at Lehigh University, U.S., has brought out an unsuspected omission on my part, to which allusion ought to be made. Though my attention was especially directed to that part of the disc, I never detected the thin elliptical ring which both those eminent observers have delineated as extending in one place across nearly the whole breadth of the South temperate zone, but which, as far as I know, has escaped the notice of other astronomers.

The present series has been carried through with my silvered speculum of 9 inches, and a power of 212: as, even on those rare occasions when the air afforded sufficient sharpness for 450, there was a want of adequate light in that ocular. The atmo-

spheric conditions were on the whole inferior to those of the preceding season, and to this cause we may refer a general feebleness in the markings of the disc, which contrasted disadvantageously with some of the former views. A suspicion, however, does exist that the cause may have lain in the planet's own atmosphere.

The colouring of different regions of the disc has continued relatively unchanged; on the whole, however, it has usually appeared less intense and marked by less contrast than previously. It must be borne in mind that the memory of the eye is no adequate substitute for actual comparison; yet it seems probable that some amount of equalisation has taken place, and from several observations it may be inferred that some sides of the globe present more plainly than others the remains of the contrast so fully developed in 1869, between the brownish yellow of the equatoreal zone and the grey purple of the North temperate belt. The North polar region has been sometimes noted of an iron-grey hue. If we now proceed to refer to other details, commencing from North, we find this cap, or clearing, which, divested of its foreshortening, must be of very considerable extent, little varied in appearance. Its streakiness is once only expressly recorded; on another occasion it is referred to as barely, if at all, perceptible, and was always very inconspicuous. The South edge of this region, however, exhibited many changes, in part physical, in part merely optical, from the various presentations of the globe. Towards the North temperate belt there was always a lighter tract, sometimes only an undefined greyish space, at others a sharp white zone. Dec. 14, this zone was of considerable breadth, and divided, nearer to its North edge, by a narrow belt, darkest in the centre of the disc, and diverging towards North in its East half, which was also somewhat wavy or ragged. This streak, a feebler companion to the great North belt, was frequently seen, but seldom in such perfection, and sometimes could not be detected; traces of its irregular form returned once or twice, but much less distinctly: it evidently occupied in strength but a small portion of the circumference, and as the season went on this region became less strongly marked.

The *North Temperate Belt*, though still a prominent feature, was repeatedly noted as considerably feebler than formerly, or approximating to the two torrid belts in colour and depth. Its character in these respects varied either actually, or from optical causes, or both. On some occasions, especially Jan. 7, I thought it had a yellowish fringe on the South side, as I had remarked more than a year before, and on Jan. 16 I fancied such an appendage on either edge. Two or three times it seemed to thin off a little towards the East

limb. Dec. 23 it was streaky, and there appeared to be a thin bright zone in its centre: this was again suspected Jan. 20, when a minute luminous stripe seemed to divide it into two very unequal portions, the narrower North; but it was a very difficult observation: this occurred during one of its browner aspects. Insulated portions of such a zone, and dark spots, were at other times more readily perceived. Dec. 21, I thought there was a lighter included portion for about one-fourth of its length in the centre of the disc. Jan. 9, a similar break intervened between two dusky, probably roundish spots, about one-sixth of length apart. Jan. 25 and 26, there were two such spots, without any perceptible stripe between them; the time of rotation shows that these must have been different, and there were probably many. Feb. 7, I suspected that there were two smaller spots, connected by a lighter space, and a larger one further advanced across the disc. March 22, there was a dark knot on the belt somewhat beyond the centre, from which a ragged projection slanted a little way North-East into the brighter adjacent space: for about one-fourth of its length from that knot the great belt seemed paler; but I could not make out with certainty whether there was a second spot where it recovered its usual tone.

The *North Temperate Zone* was still noticed from time to time to be the most luminous part of the globe, but only slightly, and not, as formerly, conspicuously so. It was also not quite so free from disturbance. Jan. 25, when there were two dark spots in the adjacent North temperate belt, I had at best moments a difficult glimpse of something like a very thin dusky loop, or inverted festoon, connecting them, and projecting across one-third of the breadth of this zone. On the following night I fancied a very narrow dark stripe across the disc close to the edge of the North temperate belt, not verified, however, some hours later. March 22, when there was, as just described, a ragged projection from this great belt to the North, I had a doubtful suspicion that the central part of the belt was attended by some very slight wispiness, or thin loops, intruding into this zone.

We now reach the *Equatorial Region*, which seemed unaltered in its proportions, and on Nov. 18 was observed to lie obviously South of the centre of the disc. Its colour, unchanged, though probably diluted, has been already adverted to. The two *Torrid Zones* were often noted as narrow, and often as equal; but the South was sometimes considerably broader and darker than the North, which, on Jan. 26, was barely visible. The markings in the included space, though seldom so clearly made out as formerly, had exactly the same form, and the 'bead-and-hollow' character was always apparent, whenever the

state of our own air, or, as often. seemed to be the case, the lateral fusion of the luminous masses, did not reduce them to a state of feebleness and indecision. The elliptical areas were sometimes so luminous on their South, and shaded, apparently, on their North sides, that with a less powerful instrument they might readily assume an imperfect zone-and-belt aspect, and as such it may be suspected that they have been sometimes delineated. The size, though not the general form, of these areas and their intervals, as far as could be distinguished, varied in different parts of this coloured girdle.

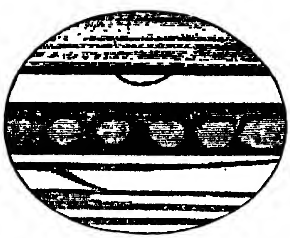
The *South Temperate Zone*, or rather its principal feature, the *South Sub-torrid Belt*, has undergone considerable change since 1869–70. At that epoch the belt assumed the form of a spiral, so gradually evolved as to be sensibly parallel to its torrid neighbour for a great part of its length, and dissipated before it reached the opposite side of the zone, or the meridian where it commenced; leaving a vacancy which was occupied by the great ellipse of Gledhill and Mayer. In consequence of interruptions, and especially of weather adverse to minute examination, I never obtained satisfactory views of the whole of this zone; but it appeared to me that after a certain degree of divergence, and subsequent parallelism as before, a curve of contrary flexure succeeded, and the belt returned to a junction with itself, a little beyond its origin. On another side of the globe, however, but how situated relatively to this intersection I cannot say, it exhibited an interesting feature in sending off a curved branch in a South-West direction across the zone, which extended convex towards South till it actually or very nearly merged in the South temperate belt. When once the air was exceptionally still (Dec. 22), it appeared to start from a small separate base, West of which was a bend in the original belt, and a minute very dark spot of perhaps $0''\cdot5$. It was seen again with another spot on Jan. 25. The belt exhibited similar variations in breadth and darkness to those noticed in the previous season, and frequently details of obviously an interesting character were too feeble for satisfactory vision or delineation.

The *South Temperate Belt* varied greatly in aspect, being sometimes divided into two parallel streaks, of which one formed a companion to the sub-torrid belt; at others broken up by white patches, which, from the general feebleness of the belt, were seldom very distinctly bounded. Not unfrequently it was so faint and diffused that very little could be made out, and a general washy appearance extended from the sub-torrid belt even as far as the Pole. Occasionally, however, the *South Polar Belt* had an independent and tolerably conspicuous existence.

Such have been the principal phenomena of this season.

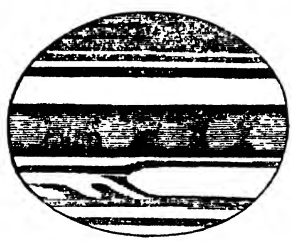
Three figures are introduced rather as illustrations than as portraits, for the original drawings were felt to convey but an imperfect idea of the minuter details, and in the process of wood-engraving the fainter portions are almost of necessity too prominent. It may be justly thought that our knowledge of this grand planet has gained but little from these observations. Still, a few deductions may be hazarded. We may safely assume that the planet is surrounded by an atmosphere charged with vapour; and if analogy has led to the supposition that the brighter stripes indicate the presence of a material like cloud, there is nothing in observation to contravene it. It has been doubted whether in the clearer intervals we actually see the body of the planet; but the great contrast between the zones and belts renders it probable that in the latter we look upon the real surface, though the much greater darkness of some of these bands seems to show that others represent only a semi-transparent atmosphere, and none possibly may be quite

Fig. 1.



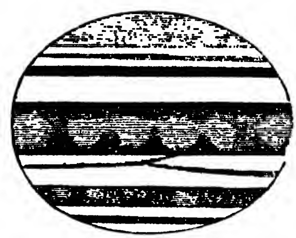
1870, Dec. 14d. 11h. 10m.

Fig. 2.



1870, Dec. 22d. 10h. 35m.

Fig. 3.



1871, Jan. 25d. 8h. 5m.

free from the interposition of a nebulous veil. There have of late been none of those very dark spots in which it is more likely that the real surface comes into view.

That the prevailing equatorial direction of these clearer tracts is caused by the swift rotation of the globe, has long been taken for granted, without, as it would seem, sufficient attention being given to the question in what way such a result would follow; and in this inconsiderate acquiescence I had long shared, till the remarks of an ingenious friend suggested further inquiry. As far as I can see, three explanations only suggest themselves: 1. There may be a certain amount of friction against some gaseous material diffused through space; but this seems improbable from the density implied in the retarding medium, and from the effect of such retardation on the diurnal period; or 2. There may be some kind of electric or magnetic polarity developed by such rapid rotatory motion: a hypothesis too obscure to be adopted as long as any other can be devised;

or 3. Such a direction may result from the different values of rotation at different heights in the atmosphere, combined with the expansion and ascent, or condensation and sinking, of the aërial strata in contact respectively with the warmer globe or cooler expanse of space: an idea supported by the analogy of our own trade-winds. But if we adopt this solution, it will lead us somewhat further. We shall find it expedient to assume a more regular distribution of heat over the surface of that globe than exists upon our own, and some consequent difference in its origin; for notwithstanding perspective foreshortening, belts are occasionally traceable at great distances from the equator. A drawing made on Dec. 22 shows a defined belt in a South latitude of probably 60° or 65°. Now since the situation of the terrestrial axis at either equinox is equivalent to the average position of the axis of Jupiter as far as solar radiation is concerned, we may suppose that if the warmth of Jupiter were derived wholly from the sun, the temperature of his equator would bear a similar proportion to the temperature of lat. 60° or 65° that the temperature of the terrestrial equator does to the mean equinoctial temperature of the parallel of Iceland; and that consequently the warming of the lower stratum of the atmosphere by contact with the globe would in all probability be too much reduced to admit of the formation of definite zones and belts; especially when we add to this the diminution to half its value of the velocity of rotation in that latitude. Hence, then, it would appear not improbable that a considerable portion of the heat of Jupiter may be of an unborrowed character. This idea, which has been advocated on other grounds, seems favoured by another circumstance. Were his temperature due merely to solar radiation, the currents ascending from the hotter equatoreal regions would be observed to deviate to some extent in an oblique direction, their lateral diffusion being unrepressed by equivalent expansion in remoter latitudes; the phenomena, however, afford but very equivocal instances of any such tendency, the oblique arrangements which are occasionally visible being too irregular in aspect or inconsistent in direction to be referred with safety to this origin. It must be admitted that the foundation of such an attempt at explanation is very insecure; still, till some more probable solution may be devised, it seems to present the least amount of difficulty and inconsistency. And as we believe that our own globe possesses a certain amount of internal temperature, independent of that derived from solar radiation, there is an antecedent probability in extending the analogy to globes of greater dimensions, such as Jupiter, and we may add Saturn, whose similarly belted surface no doubt indicates a similar constitu-

tion, and where the position of the axis enables us to trace a circular arrangement even to the vicinity of the Poles, and to infer with little chance of error, that notwithstanding the slackened rotation, arctic and antarctic belts exist upon Jupiter also.

As to the possible extent of this atmosphere, we have little to guide us. No observer, however powerful may be his optical means, has ever recorded any deviation in the outline of the limb as it traverses successively the darker and brighter portions; and yet a considerable depth would seem to be required, to admit of a difference in the velocity of rotation between the upper and lower regions, adequate to the production of so persistent a streakiness. The fact that this streakiness affects the whiter more than the yellower parts of the disc may be significant as intimating that the yellow vapours, if such they are, are of less vertical thickness, whatever may be their relative situation in the atmosphere. I am, as before, doubtful as to the existence of any such fading of the ends of the dark stripes as might arise from imperfect transparency in their clearer air. On some occasions I have believed that the North temperate belt has been fainter towards its extremities; and on Jan. 20 I thought the falling-off much more perceptible in this than in the other belts; which, if such delicate variations at the extreme verge of optical power could be trusted, might accord with the supposition of its greater vertical depth. At other times the whole of the belts preserved their tone very fairly to their extremities; and my impression is confirmed that either optical deficiency has vitiated some previous representations, or that the condition of the planet's atmosphere has been on such occasions widely different from that which it has now long maintained.

There seems little doubt that for a considerable time the North has possessed a clearer sky than the South portion of the planet. The coincidence with the period of summer in that hemisphere will be remarked: nor need the slight inclination of the axis be considered a bar to the natural conclusion, since, when the whole constitution of the globe is evidently very unlike our own, there is nothing forced in the supposition that a slight difference of inclination might be much more influential than in our own case. At present it is a mere suggestion, arising from slender evidence, and requiring further observations for its confirmation.

The most singular feature of the disc continues to be the prevalence of these elliptical areas, which it is so difficult to explain by any terrestrial analogy. That they are not confined to the equatorial region, and in fact do not there attain their

largest dimensions, appears from the very curious object delineated by Mayer and Gledhill; while some older drawings by one of our first observers, which I have been permitted to see, prove that they have been in other seasons abundantly developed in less central latitudes. At present we seem far from any solution of this mystery, while so little of a similar character can be traced among the cloud-masses of our own skies.

It may be worth consideration whether the superior brightness of the interior of the disc might lead to any inference as to the nature of the reflecting material. It is well known that the difference, though not perceptible by the eye, is very material, as we can in no other way explain the singular change occasionally noticed in some of the satellites to chocolate-colour or even absolute blackness during the time of transit. It would be interesting to know whether this very remarkable effect of contrast has ever taken place in front of the dark belts; more probably it has been confined to the luminous zones. Experiments might perhaps be devised which might show whether the more compact and regularly rounded of our white terrestrial *cumuli* exhibit any similar inequalities of luminosity, dependent on the angles of illumination and reflection. Many points of interest may probably be elucidated by other means than direct telescopic vision; and we must look forward with especial interest to the results which are, we trust, reserved for the magnificent apparatus and experienced eye of Dr. Huggins.

A comparison of the sketches taken by myself during the years 1869, 1870, and 1871, with the exquisite views of Dawes in 1857 ('Monthly Notices of R. A. S.,' xviii. 8, 50, 72), presents so much similarity in various respects, as to lead to a curious suspicion that the whole atmospheric shell of the planet, without any material disturbance in its relative arrangements, may have been shifted Southward by more than half the breadth of the equatorial zone; the South torrid belt, identified by its attendant festoons, occupying at that time a position very near the centre of the disc; while several of the characteristic features of what we have termed the South temperate zone and belt are to be recognised in a corresponding position of greater remoteness from the Pole. So lively was the first impression of this that I thought it might have been at once accounted for by a reversed presentation of the planet's axis; but on considering the dates, as well as the position of a satellite sketched by Dawes in transit, I found that no such explanation could be admitted. However little weight may be due to such a comparison, it suggests, at any rate, the desirableness of micrometric measurements of the latitude of the principal belts during future oppositions.

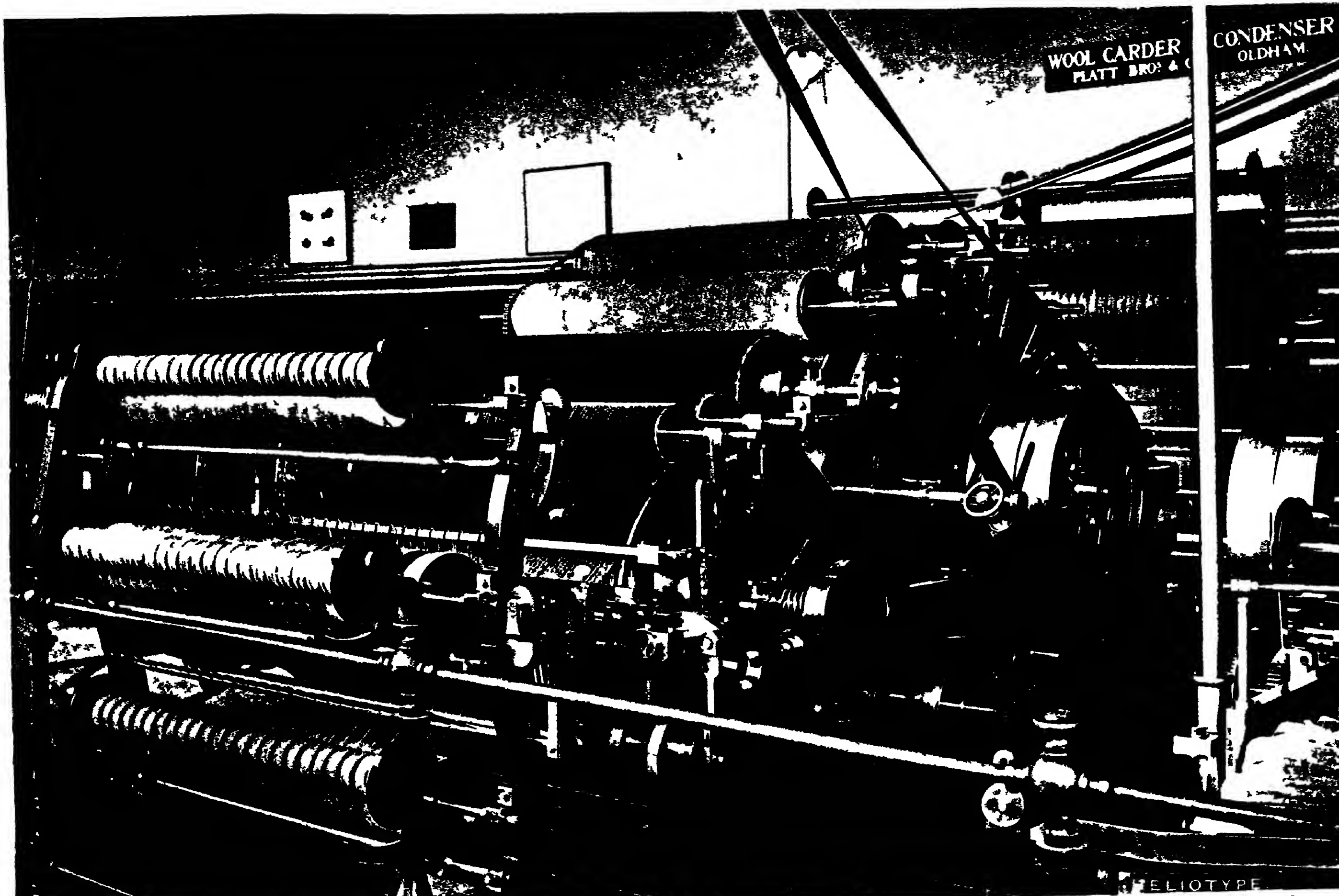
# THE INTERNATIONAL EXHIBITION AT SOUTH KENSINGTON.

By S. J. MACKIE, C.E.

[PLATE LXXV.]

THE present Exhibition is by no means comparable with the magnificent affairs of 1851 and 1862 in this country, and in France in 1855 and 1867. The professed purpose of the annual Exhibitions at South Kensington, now understood to be inaugurated, is the illustration of the special progress made in particular sections of manufactures and science in respective decades of years. In the present—the first of the series—woollen materials, machinery, and fabrics, are supposed to be specially represented. Pottery is also an item. These two groups of manufactures and industries are expected to be on their trial, and the advance made since 1862 to be recorded in the reports which are to be published. Pictures, sculptures, and the various illustrations of the fine arts, as well as inventions, will always find admission, and, like the flowers in the Horticultural Gardens which the Exhibition buildings inclose, will blossom in full vigour year by year. The second and following Exhibitions will be devoted specially to other branches; until, after a decade of Exhibitions, the return will be made to woollen and pottery, and then will recommence another series of similar competitions.

The Horticultural Gardens form nearly a square area, containing about twenty-two acres; and the present permanent Exhibition buildings have been erected along the east and west sides, in blocks of two storeys each. At one end of the gardens portions of the exhibits are located in corridors leading to the Albert Hall, in which also a great number of pictures, fabrics, educational and other articles, are arranged. Across the opposite end of the Gardens there are temporary buildings, where the Meyrick collection of armour, military weapons, astronomical and other instruments, are displayed. The two


WOOL CARDER
PLATT BROS & C
CONDENSER
OLDHAM.
ELIOTYPE

subjects which form the text of our present brief review will be the machinery for woollen fabrics and the new inventions.

In regard to the woollen machinery, the whole of the processes, from the animals producing the raw material to the finished cloth, are represented, and the entire range of woollen manufacture can be followed through by visitors, under the guidance of anyone versed in the subject. The necessity of locating each exhibitor's goods by themselves renders impossible that arrangement in consecutive order which would enable this to be done without such assistance, although this has been partially attempted. The animals include some foreign ones sent by the Zoological Society and Miss Burdett Coutts, and British sheep sent by Mr. Wallis and others.

The first process in respect to the actual manufacture of wool is the washing of the sheep to cleanse its fleece. For this purpose the apparatus contributed by Messrs. Gwynne—a sort of water-pipe cage—is admirably suited, within which the sheep is placed. These pipes, forming the frame of the cage, are perforated, and through them numerous small streams of water, raised by one of their excellent centrifugal pumps, intersect inwards, pouring in all directions upon the animal. As the water issues under some considerable pressure, the washing is much more thorough and effective than it could possibly be by any amount of hand labour, and the machine also saves one man's labour in holding the sheep. The next operation in the series is the shearing—the taking the raw material off the back of the animal. This also is illustrated by another machine by the same eminent firm. As a pump for raising water for such purposes, the direct-action pump of Messrs. Hayward, Tyler & Co. also merits attention.

We have next displayed the raw material in samples of numerous kinds, spread all about the Machinery Court. There are good and bad sorts, as a matter of course; but what we have mainly to do with, in respect to the machines required to work the wool, is the length of the staple or fibre. This is short, or long, or short-long, as it may be called. For example, short is wool under, say an inch and a half; short-long is, say three inches; and long may be six inches, or even more, in length. The machines are usually made to suit these respective qualities. The short wool may be passed direct to the "burring" machine, in which it is subjected to a loosening and shaking action in a rapidly revolving cylinder, and thence through a series of carding cylinders, the object being to get out the "burrs" or seeds and other hard substances which adhere to, or get mixed with, the sheep's fleece. After the wool has been scoured—Mr. Petrie's machine typifies this process—it is partially dried, and the wool then has to be passed through an oiling process to soften and straighten

the fibres. A Belgian machine for this purpose is shown in the Exhibition by Messrs. Curtis, Parr & Madeley. This carding process has to be continued through a series of carding-engines, in order to free and clear the wool sufficiently for spinning into yarn. These machines consist of an arrangement of cylinders coated with a very even brush-work of fine wires, called cards, which, working in contrary directions, pull open the wool and lay the fibres evenly in the direction of their length, the foreign substances falling away the more entirely the more perfect the machines and the more thoroughly they perform their operations. The three carding-engines displayed by the Messrs. Platt, of Oldham—the finest machinists of this class in the world—will be regarded with admiration. The first of these carding-engines is called the "scribbler;" the second the "intermediate;" the third "the finisher and condenser." We have selected their "finisher and condenser" for our illustrative plate, as the type of the best English machine of its class ever produced.

This machine is in itself a study. The "sliver" is brought over from the second carder by a Scotch feed, and is laid beautifully on the lattice for feeding the machine. The wool then passes over the cylinder, being constantly combed or carded by the five pairs of smaller rollers called "workers" and "clearers." On coming finally from the "doffer," the web, which is then the whole breadth of 48 inches, is divided into 74 threads; and that part of the machine which now gives the slight roll or twist to these threads is most worthy of close attention, as an admirable mechanical motion. It is seen prominently in the front portion of the machine in our plate. There is a novel addition also in this machine of two small "dicky rollers," the cards of which go just a little way into the card covering of the doffer, and prepare it beautifully to receive the wool from the cylinder.

When the wool comes from the machine it has the appearance, but not the reality, of threads; the fibres simply cling together, and it still requires to be firmly twisted, or spun into "yarn." This is done by the "mule;" and here again with pride may the superb workmanship of the famous Oldham house be referred to. This machine, with its 192 spindles twirling round so fast that their rotation is invisible; the whole row of them coming forward, drawing the thread; stopping; spinning; winding up and running in on the bobbins; coming forward again to spin, wind, and return; is a sight of which one never tires, and over which one never forgets the memory of the man who gave this wonderful and most useful machine to the world.

We must here, in justice to its merits, refer to the machinery

of Mr. Martin, of Verviers; and it will be well also to understand some of the distinctions between the English machines and those of that active iron-working country, Belgium. We see in the Belgian machinery less finish, less admirable workmanship, but useful good engines nevertheless. We see in the carding-engines that the cards are felted, and consequently less of the card-wires are exposed, the cards being consequently stiffer. Mr. Martin's condenser is a novelty, and has excellent qualities; the web is parted by figure-of-eight straps running over two sets of cylinder-rings, the intersection of these straps acting like scissors in dividing off the threads. No space is thus lost in dividing the web into threads. The wool for these engines should be well cleaned—a process in which the Belgians surpass the English.

The combing-machines of Mr. Walmsley demand special attention for the novelty of their principle—the circular rotating comb. There is nothing more thoroughly novel in principle in the entire court, and in every way they appear to do their work well. The combing process is of course allied to carding, the object being to get the wool perfectly cleared and disentangled, and the long fibres separated from the short. This is admirably done in these machines by rollers, which draw the long fibres from the comb and leave the short ones in it, to be taken off by another set of rollers. In this way continuous slivers of long wool, and of short wool and waste, are simultaneously delivered into separate cans.

The spinning of the yarn into thread is only shown by Mr. Smith's spinning machine: there are, however, other processes, such as drawing and roving, which are not exhibited. We have traced the wool now up to the state of preparation required for the final production of textile fabrics, and have arrived at the stage when it passes to the loom to be woven into cloth, tweed, and such like goods. The looms are of different kinds, according to the purpose for which they are to be employed. The plain loom has one shuttle on each side, and works with ordinary pickers, and a common tappet-heald motion; the fancy looms have shuttles up to four in number, and work with the Jacquard and chain-picking motions. After this we have—so far as the fine wools passed through the carding engines which we have been noticing are concerned—finally to take a glance at the "cloth-finishing machines," or those employed to surface the goods with a fine pile or nap. Mr. Ferrabee illustrates this process in a very practical manner.

The coarser kinds of wool, and the fibres of old fabrics, are also used for making felts and carpets, and other of the less fine goods; and looms for carpet-weaving are well represented.

Mr. Hall, of Bury, stands foremost in this division; and his Brussels' carpet loom is remarkable for the simple, ingenious, and effective application of the magnet for drawing over into position for re-inserting the weft wires which keep the loop or "pile" up during the process of weaving.

Space warns me to turn to the "Inventions." Some, indeed most of them, are well known to those who stand to the front of general knowledge; but to the great majority of mankind they will be at least novelties previously only heard or read of. The most notable of these, for any general and wide-spread practicable purpose in ordinary daily life, is that special application of photography for book illustrations called "heliotype" which we have ourselves used for the production of our illustrative plate. The process is a very simple but most effective and practical one.

Of all the shortcomings of all previous processes and attempts in this direction—the application of photography to book-work—the most serious and vexatious, and the most obstructive to commercial purposes, has been the dependence upon sunshine. Silver prints, each one printed by a separate act involving a separate and uncertain interval of time, with results in the pictures produced the very opposite of uniform, were found to be utterly impracticable. Then followed a series of very ingenious and, in some cases, very beautiful methods, but in all which there have existed obstacles to production in commercial quantities and with requisite commercial rapidity and uniformity. This has been perfectly accomplished by heliotype—the prints being by it printed direct from the ordinary printing-press by ordinary printing-ink. The impressions are thus as rapidly taken off as lithographs, and are as permanent and as uniform as the most approved classes of book or plate-printing. The process may be easily understood. Chromate of potash is sensitive to light, hardening in proportion to its intensity and the time of exposure; chromalum renders gelatine insoluble. Gelatine, when mixed with chromalum and chromate of potash, becomes a compound sensitive to light and insoluble in water. It is not, however, impervious in the mass; but the parts hardened by light are so. If, then, a photographic negative taken by the camera be placed over a thin sheet of this prepared gelatine, an invisible picture is hardened into its very substance, and this picture being impervious to water in proportion to the degree of hardening, rejects water like the greasy drawing on a lithographic stone; the mass of the gelatine plate, however, absorbs the water like the body of the lithographic limestone; and so, just as in lithographic printing, the picture takes the printers' ink from the printing-roller, whilst the plain water-

wet portion rejects it. There is, however, this difference in the heliotype plate, that it is somewhat of a spongy nature and swells, leaving the picture also more or less depressed according to the depths of the tints as well as proportionately adhesive to ink. The rapidity with which this process can be applied was well exemplified in the present instance. The order to take the negative was given to the Stereoscopic Company at the Exhibition at 8 A.M.; the negative was delivered to a messenger at 10 o'clock, sent to Willésden Lane, where the Heliotype Works are located; two hours were taken up by telegraphic communications, through an accidental omission in forwarding instructions for dimensions, the negative having been taken on a large glass; the proof was sent for inspection by post, returned, and the printing of the plates for this journal was actually commenced at ten o'clock the next morning, being regularly continued at hand press until the whole required number was completed. Ten thousand impressions can be printed from a single gelatine plate without deterioration, and of course any number of gelatine plates for working at any number of presses can be taken from the same photographic negative; and these being done during the same period would, of course, produce results almost exactly identical.

Turning to other less familiar applications of inventive skill, we may take Admiral Inglefield's Hydrostatic Steering Gear, which has been applied to some of her Majesty's large iron-clad ships with perfect success. To steer such ponderous monsters is very far from being easy; in rough weather from twenty to even forty men are required at the tiller, and the force of the waves will even then sometimes cast the whole of them adrift. At the bottom of a deep ship such as these there is the great hydrostatic pressure due to the twenty or thirty feet of water in which the ship swims. Admiral Inglefield uses this as the source of a most powerful intermittent force. He allows the water to come into cylinders, and to work pistons within them much after the manner steam would do in an ordinary engine; and thus he acquires, in small hydraulic rams attached to the tiller, a motive power equal to 1,000 lbs. to the inch of surface. A single man now, by this method, can steer the largest ship in the wildest weather. This example typifies admirably the class of inventions brought to a practical application.

In Mr. Tommasi's model for utilising the tides as a source of power for machinery, we have an idea not yet practically realised, and typical of another class—unrealised inventions. Men long ago have looked to this solemn source of enormous but totally

neglected power. The model in the Exhibition is a rough one, and the power exerted small. The idea is, however, at this moment like what the embryo windmill steam-engine once was—well worth pondering over. It must, however, be constantly met with antagonistic considerations, and especially must its probable results be compared with the known effects realised by steam. Mr. Tommasi's model presumes that a large air-tight reservoir could be built in which the water of the tide, flowing through a pipe and rising inside, would compress the air within. This condensed air would be drawn off as required in the service of the engines. Now two considerations instantly present themselves. First, the tides rise and fall in very few places to the extent of twenty feet. Twenty feet head of water would give the compressed air only a pressure of 10 lbs. on the square inch. Steam works up to 60 and 70 lbs. pressure, and, therefore, as a motor, would be six or seven times superior. But steam costs constantly money for the supply of fuel to keep it up; the tides do their work for nothing. We have then the interest of the money cost of the reservoir to put against the cost of the coal burnt. A five-horse power engine would consume some sixty tons of coal in a year's incessant work; the cost of a tidal air-compressing reservoir for a five-horse power engine would be, it is said, 800*l.*; the interest of this at 5 per cent. would be 40*l.* We might reckon the coal at 15*s.* per ton, or 45*l.* One does not see the economy of air. This line of thought, however, should not be condemned. To utilise the tides would be a world-wide benefit; but the idea has not yet been contemplated long enough or deeply enough for universal application. There is in all probability a way to win.

Thomson's road-steamer, with india-rubber tyres; Hodgson's wire tramway, with the saddles of the buckets clinging on to the wire-rope by simple adhesion; Girdwood's copper-wire steam-packing, the condensation of water within which forms the lubricant; Siemen's electrical pyrometer, for measuring the degrees of very high temperatures; Michele's cement-testing machine, in which the bent lever is most ingeniously applied; Captain Scott's selenitic cement; are other examples of really useful and practical inventions, each of which might well have a page to itself to do justice to it. Mr. Galloway's and Mr. Warsop's aerated steam is a topic also well worthy of close investigation and consideration.

Amongst instruments of precision and fine philosophical workmanship, very notable are Sir Joseph Whitworth's instrument for measuring to the one-millionth part of an inch, and the astronomical apparatus of Messrs. Cooke, of York.

In a short notice like the present one cannot deal in detail

with many objects, but it is useful at least to give indications of some of the most prominent and instructive. Such a simple record serves to commemorate the occasion, as well as to lead many to see and to note carefully subjects which otherwise might be passed by, or missed by accident or the want of looking for.

# REVIEWS.

## THE DESCENT OF MAN.*

It may be necessary at the outset to state that, in postponing our notice of the present work to this number of the Review, we had fancied that the work was of far greater importance than it is. For great as the labour may have been on the part of the author, of collecting and putting together so vast an accumulation of facts, we should not be just to our readers did we not confess that the volumes are in no respect to be compared with either of Mr. Darwin's previous books. In point of fact, we might readily have noticed this work in our previous issue, had we not thought that it was something like its predecessors, and on that account determined to deal with it slowly, and at our leisure. It must not, however, be imagined that the work is not in every way worthy of the author, for it is a most important treatise, and is full to overflowing with facts which, less or more, help to prove the author's case.

What we mean is, that as regards the descent of man the volumes somehow or other contain less than we had expected of them, and, as regards the arguments they set forth, the author's case seems to us but little stronger, if anything, than before. The reader must not assume from this that we hold Mr. Darwin's theory to be in error. Far from this; for we are convinced that his views, taken altogether, are strictly and rigidly true. We are as satisfied that man came from some species of monkey, rather than from a heap of unorganised dust, as it is possible for us to be. That which we assert is, that Mr. Darwin's book is not so convincing to the general reader of the force of this idea, as we had imagined it would be. It is full of details which the naturalist can value, and can see how every one of them convinces him more and more of the origin of species by natural selection, rather than by any other means. But to the general reader it is a heavy book, without sufficient thread of continuity to give it adequate effect in his mind.

And yet it must be admitted that it contains nearly all the evidence upon the subject, and in some cases put in a very strong manner indeed. But for all that, we fear that the volumes will fail to convince those who are worth convincing as to the origin of man. Yet how little is on the other side, absolutely nothing in the form of legitimate reasoning; and still the

* "The Descent of Man, and Selection in Relation to Sex." By Charles Darwin, M.A., F.R.S. 2 volumes. London: John Murray, 1871.

Darwinian opponents call upon the author almost to show them, by ocular demonstration, the truth of his views. Of course such a demonstration would be absolutely impossible, for those changes which Mr. Darwin supposes to take place have occupied millions of years in their performance, step by step. It is remarkable that of the various opponents which Mr. Darwin has raised up to his views, most of them consider that the shaping of an implement for use is not only peculiar to man, but must be so. And, after all, is there not much truth in Sir John Lubbock's suggestion, that when primeval man first used flint stones for any purpose, he would have accidentally splintered them, and would then have used their sharp fragments. "From this step it would be a small one to intentionally break the flints, and not a very wide step to rudely fashion them. This latter advance, however, may have taken long ages, if we may judge by the immense interval of time which elapsed before the men of the neolithic period took to grinding and polishing their stone tools. In breaking the flints, as Sir J. Lubbock likewise remarks, sparks would have been emitted, and in grinding them heat would have been evolved; thus the two usual methods of obtaining fire may have originated." Surely this is nothing but probability, and no sane person can object to reasoning conducted on so fair a scale. It is not too much intelligence to expect from anything superior to a modern ape or baboon. Even animals lower in the scale possess very nearly power enough for this. "No one," says Mr. Darwin, "supposes that one of the lower animals reflects whence he comes or whither he goes—what is death or what is life, and so forth. But can we feel sure that an old dog, with an excellent memory and some power of imagination, as shown by his dreams, never reflects on his past pleasures in the chase? and this would be a form of self-consciousness. On the other hand, as Büchner has remarked, how little can the hard-worked wife of a degraded Australian savage, who uses hardly any abstract words, and cannot count above four, exert her self-consciousness, or reflect on the nature of her own existence." Really these observations are very true; they lead us to make comparisons between the highest civilised man and the lowest savage, and to confess that the gap intellectually, if not structurally, is very great indeed.

Mr. Darwin attempts to trace the backward career of man; and although he does not bring forward a massive case in its favour, he urges some evidence that is of a serious nature. He says that the most ancient progenitors in the kingdom of the Vertebrata, at which we are able to obtain an obscure glance, apparently consisted of a group of marine animals resembling the larva of existing Ascidians. These animals probably gave rise to a group of fishes, as truly organised as the Lancelet; and from these the Ganoids and other fishes like the Lepidosiren must have been developed. From such fish Mr. Darwin thinks a very small advance would carry us on to the amphibians. Birds and reptiles, he has shown, were once intimately connected together, and the Monotremata now in a slight degree connect mammals with reptiles. But no one can at present say by what line of descent the three higher and related classes—namely, mammals, birds, and reptiles—were derived from either of the two lower vertebral classes, namely amphibians and fishes. In the classes of mammals the steps are not difficult to conceive which led from the ancient Monotremata to the

ancient Marsupials, and from these to the early progenitors of the placental mammals. "We may thus ascend to the Lemuridæ; and the interval is not wide from these to the Simiadæ. The Simiadæ then branched off into two great stems, the New World and the Old World monkeys; and from the latter, at a remote period, Man, the wonder and glory of the universe, proceeded."

Of the manner in which Mr. Darwin supports the argument which is stated as above, the reader must judge for himself. All that can be urged in its support is brought forward, and that only as Mr. Darwin can adduce it. But the evidence is not absolutely a great deal, though relatively it is over-poweringly strong, and so we leave it to those who will take up the volumes for themselves.

Of the difficulties of the argument none are more familiar to anyone than Mr. Darwin, as the following passage, in which the principal difficulty is fully admitted, will amply show:—"If, however, we look to the races of man, as distributed over the world, we must infer that their characteristic differences *cannot* be accounted for by the direct action of different conditions of life, even after exposure to them for an enormous period of time. The Esquimaux live exclusively on animal food; they are clothed in thick fur, and are exposed to intense cold and to prolonged darkness; yet they do not differ in any extreme degree from the inhabitants of Southern China, who live entirely on vegetable food, and are exposed, almost naked, to a hot glaring climate. The unclothed Fuegians live on the marine productions of their inhospitable shores; the Botocudos of Brazil wander about the hot forests of the interior, and live chiefly on vegetable productions; yet these tribes resemble each other so closely, that the Fuegians on board the *Beagle* were mistaken by some Brazilians for Botocudos. The Botocudos again, as well as the other inhabitants of tropical America, are wholly different from the Negroes who inhabit the opposite shores of the Atlantic, are exposed to a nearly similar climate, and follow nearly the same habits of life."

Thus we see with what fairness and honesty Mr. Darwin tells of the facts against himself as well as in his favour. Of the contents of his volumes we can only say that they are extremely interesting, and they all go toward his theory of man's origin. But he has not got a clear case, though all the testimony is with him and none on the other side; it is misty and complicated, and we do not think that the mass of naturalists will accept some of the conclusions which we have extracted. What they will say will probably be this: "You are right as to your theory of man's origin; he undoubtedly has come from the monkey class, but we cannot accept your transition line as perfect, and we somewhat regret that you have drawn it so far at present."

## THE BEGINNING.*

THERE are among the lower strata of the world of scientific men certain persons who, without a sufficient knowledge of what has been done, have yet very definite and distinct views, which they imagine are all rigidly true and correct. Such men are generally looked up to by the inferior class among whom they travel, and they very seldom go amongst their superiors in knowledge. Of such a class astronomers are, to those who have to do with journalism, a tolerably familiar group. There is the man who, with a certain knowledge of geometry, has sought to prove that the earth stands still, and the sun does the work which most of us attribute to the earth. Then there is the author who delights to find comets of a substantial character, and the man who proves finding the sun's distance by no means like what it is represented to be, and so on. Now, of a similar class—but it must be confessed with much more learning at his hand, and with a far greater degree of reason at his call—is the author of the present volume. Mr. Ponton is not of the lunatic class of most of the authors to whom we have referred. His book is only a little unreasonable; it is by no means badly written, and to the general reader it contains a considerable deal of information. But so far as anything novel is concerned, the book is absolutely and completely barren; it has not a single fact that is new, nor anything worthy of serious consideration in the shape of ideas or reflections. Nor does the title convey a proper idea of the nature of the book, for it has very little to do with the beginning; and what the author's aim has been in accumulating together, in a book ostensibly upon the universe, such a series of plates respecting *diatomaceæ desmideæ*, and their allies, we cannot for a moment imagine. It seems to us as if the author was a microscopist; and that, having written a work on such a gigantic subject as the universe, he thought it a pity not to make it include the sum-total of his labours. Such, it appears to us, must have been his idea; and if the conjecture be correct, we cannot blame him too much for so utterly senseless a proceeding. We must do Mr. Adlard the justice to say that the plates in the volume are exceedingly well drawn, and are very good representations of the structures they are intended for; indeed, *tout entier,* this is the best part of the work.

Of the contents of the book we know not well what to say. The first few chapters—on the antiquity of matter, the terraqueous globe, solar energy, &c.—contain few blunders, and are well written, and interesting general reading. Of course, in regard to the sun, the matter is comparatively speaking old, and the information conveyed inaccurate in some cases; but altogether this part of the volume is not so bad. When we come to the more essentially animal part of the volume, we find the author putting forth his own ideas more freely and more frequently. It is true that a greal deal of this part of the book is taken from that admirable treatise of De Quatrefages, on the "Metamorphoses of Man and Animals," and so far is

* "The Beginning: its When and its How." By Mungo Ponton, F.R.S.E. London: Longmans, 1871.

excellent; but a great deal is taken from other less authoritative works, and is mixed together with the author's own speculations. Finally, the last part of the work deals with the author's own ideas on the first chapter of Genesis, and treats upon its successive creative epochs and the divine authority of the Hebrew records. On this portion we must distinctly decline to enter.

A few quotations will represent what we have said of the author's original ideas better than any words of ours; and they will put the reader, too, more thoroughly in acquaintance with some of Mr. Ponton's notions. In endeavouring to prove the existence of a human soul, he argues as follows: "Take the familiar case of human thought. Man cannot exercise his intellectual any more than he can his physical powers, without food; but the food ought not, therefore, to be regarded as the origin or cause of his thoughts. Neither can thoughts be deemed the representative of the food. The two things are incommensurable, and incapable of mutual comparison. It cannot be affirmed that a pound of food will produce so much thought, in anything like the same sense in which we can say that a pound of food will produce a certain equivalent of muscular action; or that a pound of fuel will produce so much heat, which may be employed to raise a certain weight a foot high. The food, in its relation to thought, acts somewhat like the spark applied to the powder magazine. The vast mechanical force attending the explosion represents the motive energy, not of the tiny spark, but of that statical power which was treasured up in the gunpowder. So human thought does not represent the food digested in the human stomach, *but is the result of the innate power with which God has endowed the living being man.* It is in the motions requisite to maintain the mechanism of his organisms, and those which he voluntarily performs, that we are to seek the equivalents of the physical energy imparted by the food which he digests and oxygen which he inhales. But his thoughts are something beyond all these, and *are the manifestations of man's inherent powers as a rational living being.*"

We do not care to waste space in analysing the preceding passages, for any of our readers can do it for himself. At the very utmost, all the writer was justified in asserting was, that he did not know anything further about the question of thought; but the manifest absurdity of the argument in favour of a soul is clear enough, from the fact that it applies with as much force to a parrot or a crocodile as to man. Another, and somewhat more extraordinary effusion of Mr. Ponton, may be found in his observations on the coal measures, which he supposes to have taken their origin long before the period of the formation of the sun. He says: "Looking, then, to the peculiar characters of the vegetation buried in the coal formation, to the difference in the conditions of climate which must have prevailed on the globe while it flourished, and to the immense remoteness of the epoch when those needful conditions were likely to have subsisted, it becomes a highly probable conclusion, that *that earliest vegetation clothed the surface of the earth long before the sun had begun to shine upon our globe*—being sustained by those luminous and thermal vibrations which have been shown to have partially existed as the earliest of all physical phenomena, and before the establishment of any centres of motive energy." To this geological theory we have only to express our utter amazement at the marvellous workmanship of the coal formations

and the fossils which it contains, sustained by such peculiar showers of light as the author imagines. One of the most interesting chapters is that headed Protoplasm. In this the author endeavours to attack Professor Huxley's well-known essay in the "Fortnightly Review" (February, 1869), and less or more to side with its chief opponents, Professor Lionel Beale and Mr. J. H. Stirling. This chapter we shall not dwell on, as we fancy the reader, whichever side he takes, will be sure to find it out and read it for himself. If he reads it carefully, it is utterly impossible not to see how the author confuses and confounds Professor Huxley's argument, and after all gives nothing but plain point-blank assertion in opposition. The reader will be amazed, too, to observe that, after using Beale and Stirling against Huxley, he himself comes to some conclusion different from both, and which appears to us to support the notion of a sort of spiritual organisation which is standing up within the ordinary physical being. This part of the work is the most palpably ridiculous of the whole, and having noticed it, our readers must expect us to go no farther, but to leave the work to them, in the faint hope that their verdict may be less severe than ours.

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## POPULAR ASTRONOMY.*

ASTRONOMY is very nearly taking the place which years since was occupied by the aquarium and marine zoology. The spectrum has lent it quite a novel interest, and so much interesting work has been done upon the constitution of the sun within the past ten years, that really astronomy seems likely to have as great a hold upon the popular mind as any of the lighter scientific pursuits. The work before us puts forward, as a special claim to popular favour, that it is written in a style to attract the general reader; and further, that it contains none of those algebraical studies which are common enough in treatises on astronomy, and which frequently drive away from the study of the stars those who do not understand either algebra or geometry, but who would otherwise gladly learn the constitution of the heavens. Such in general terms is the argument of the author, who attempts to put astronomy before the reader in as simple and intelligible a manner as possible. Still, we must confess that, to our minds, he has not been very successful. He has written a book, no doubt, which contains nought but what has been over and over again taught in some shape or other; but his style is, in our opinion, utterly unsuited to a popular writer. His sentences are very long, some extremely so, and his language is by no means simple; so that though his book might well be read and advantageously studied by the fully educated man, we fear much for the great mass of readers to whom his work is addressed. Indeed, in this respect it falls far short of Mr. Proctor's splendid work upon the sun, which we recently noticed, being neither in the novelty of its matter, nor in the ease of its style, in the

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* "Astronomy Simplified for General Reading," with numerous new Explanations and Discoveries in Spectrum Analyses, &c. &c. By F. A. S. Rollwyn. London: William Tegg, 1871.

slightest degree comparable with that splendid volume. As regards the method of description pursued by the author, we admit freely that he pursues his plan of teaching without the aid of mathematics; but still his pages are not light reading, however accurate they may be. Here is a fair specimen of his work; it is from his chapter on the sun:—

"The sun exhibits every characteristic evidence of a body enveloped in an atmosphere of flame, the lower part of his atmosphere being comparatively dark, coinciding with that portion of the flame of an ordinary candle, or other body under combustion, intervening between the brightest portion of the flame, or region of white light, called the *photosphere*; and above that a region in which coloured flame or light is sometimes manifested, especially along the edges of the solar disc, and which last region is called the *chromosphere*. But for a singular peculiarity of the solar disk, however, to which great interest and attention have been of late years attracted, we should probably never have been able to discover that the solid matter of the sun was not co-extensive with its apparent dimensions or luminous appearance, or to have known, as we now definitely do, that the real body or solid mass of the sun is a dark sphere of matter confined within a fiery prison-house—a robe of fiercest flame. The peculiarity we refer to is what are popularly called the spots in the sun, an obvious misnomer, as we shall soon perceive, but a characteristic enough description of the appearance presented."

The foregoing passage conveys a rather favourable idea of the author's style, and we have selected it in consequence, yet it possesses less or more of the qualities to which we have alluded. Of course the book is not advanced in the knowledge which it conveys. For instance, the multitude of facts conveyed by the spectroscope are left untold by the author, who, however, has something to say upon the subject of spectroscopic matters. It is this part of his volume—the only part which has anything new in it—that we have to object to. We do so because it contains ideas which are at all events entirely new. But we object to it all the more because we believe the author's opinions to be utterly unsound, and to have nothing whatever in support of them. We think, therefore, that they should not have been introduced here, but in the "Monthly Notices." The author holds the singular belief that "the different colours of the spectrum are only different degrees of intensity in the manifestation and action of light—the blue being the weaker, the red stronger, and the yellow the strongest, short of white light." He endeavours to support this idea in these pages, but his arguments are of the weakest possible nature, and altogether the subject is misplaced. The illustrations are many of them excellent, and are exceedingly numerous, but some of them are rather fancifully coloured and shaded. Finally, the religious tone of the book is too constantly present, and is, to our minds, a good deal too one-sided, and we think we may add, too bigoted also.

## A METEOROLOGICAL TEXT-BOOK.*

THE Secretary of the Scotch Meteorological Society has supplied us with a small text-book on the science of meteorology which is at once plain, well worked out, and exceedingly full of interest. Indeed now-a-days, when we are deluged with works, six or seven of which are invariably compiled from some pre-existing volume, the work of Dr. Buchan is a most refreshing one, and we think he deserves considerable credit for the labour he has taken in producing the volume. The contents of the work are briefly as follows:—History and scope of the science; weight or pressure of the atmosphere; distribution of atmospheric pressure; the method of observing and calculating temperature; solar and terrestrial radiation; distribution of terrestrial temperature; relation of temperature to pressure; moisture of the atmosphere; mists, fogs, and clouds; rain, snow, and hail; prevailing winds; monsoons, local and other winds; storms; atmospheric electricity; thunderstorms; whirlwinds and waterspouts; aurora borealis; ozone, optical phenomena; meteors; and lastly, weather and other warnings. All of these chapters are full as they can be of useful matter, and must be read by those who delight in such matters. We may, however, take one or two notes from them. Firstly, of the box for thermometers. This is particularly useful, even to the most limited weather student. It is necessary that thermometers should be protected "from the direct and reflected rays of the sun, and at the same time have the benefit of a free circulation of air. No possible arrangement can completely fulfil both these conditions; for if they be completely protected from solar radiation, the circulation of the air must be unduly interfered with; and if the circulation of the air be quite unimpeded, the thermometers are unduly exposed to radiation. All, therefore, that can be secured is a compromise between protection and circulation. The best and cheapest contrivance yet devised to meet these requirements is the *louvre-boarded box for thermometers*, constructed by Mr. Thomas Stevenson, C.E., Edinburgh, and now largely used by the observers of the Scotch Meteorological Society and other meteorologists." The author then gives a minute description of the box, accompanied by figures, and describes how it is to be placed, and the best method of taking observations with it, all of which we commend to the serious consideration of young meteorologists. Similarly we would recommend his attention to the *evapometer*, the *atmometer*, and the *hygrometer*, on each of which the author has some telling remarks to make. Under the head of rain, snow, and hail, we find the various descriptions of rain-gauge described, the author seeming to imagine that the invention of Mr. G. L. Symons, of London, who is so well known for his meteorological investigations, is about one of the best. We find under this section a short account of those regions of the earth in which there falls no rain from year to year; such are the coast of Peru, the valley of the rivers Columbia and Colorado, the Sahara, and the desert of Gobi.

* "Introductory Text-Book of Meteorology." By Alexander Buchan M.A., F.R.S.E. William Blackwood & Sons, 187 .

On the subject of halos and the colours of clouds the author makes some remarks of import. Altogether, the book seems remarkably well put together. Without affectation of style, but with a serious earnestness of purpose, the author appears to us to have gone about his work, and therefore that he has thoroughly succeeded need not surprise us much.

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## THE SUB-TROPICAL GARDEN.*

WHETHER the term sub-tropical be familiar enough to gardeners and those connected with the culture of flowers we know not, but we think that the author of the work before us would have done better had he employed some expression which better conveyed his meaning. However, as he has expressed the signification of the term in his preface, we may as well give it to our readers. Sub-tropical gardening, he says, means "the culture of plants with large and graceful, or remarkable foliage or habit, and the association of them with the usually low-growing and brilliant-flowering plants now so common in our gardens, and which frequently eradicate every race of beauty of form therein, making the flower-garden a thing of large masses of colour only." We confess we are not very well satisfied with this definition of what sub-tropical implies, but we suppose we must accept it, though it clearly does not imply the plants of any particular tropic or division of the globe. Mr. Robinson appears to be one of the very few gardeners of the present era who have the slightest possible degree of taste. Hence we find him at variance with most of the existing race of gardeners; and in his book he sets out his plan of grouping large and small plants, trees, and shrubs, and ordinary garden flowers, in such a manner as to produce a harmonious and a handsome result. We cannot help uniting in the author's remarks upon the abominable style in which our London parks are laid out. Speaking of the lumpish monotony of gardening, he says: "It is fully shown in the London parks every year, so that many people will have seen it for themselves. The subjects are not used to contrast with, or relieve others of less attractive port and brilliant colour, but are generally set down in large masses. Here you meet a troop of Cannas numbering 500, in one long formal bed; next you arrive at a circle of Azalias, or an oval of Ficus, in which a couple of hundred plants are so densely packed that their tops form a dead level. Isolated from everything else, as a rule, these masses fail to throw any natural grace into the garden, but, on the other hand, go a long way towards spoiling the character of the subjects of which they are composed. For it is manifest that you get a far superior effect from a group of such a plant as the Gunnera, the Polymina, or the castor-oil plant, properly associated with other subjects of entirely diverse character, than you can when the lines or masses of such as these become so large and so estranged from their surroundings that there is no relieving point within reach of the eye. A

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* "The Sub-Tropical Garden; or, Beauty of Form in the Flower Garden." By W. Robinson, F.L.S. London: John Murray, 1871.

single specimen or small group of a fine Canna forms one of the most graceful objects the eye can see. Plant a rood of it, and it soon becomes as attractive as so much maize or wheat. No doubt an occasional mass of Cannas, &c., might prove effective—in a distant prospect, especially—but the thing is repeated *ad nauseam*." In these observations we entirely concur, and we trust the author's influence may extend, so that we may hope for intelligent gardening in a few years. This book, at all events, must do something towards the conversion of existing gardeners. It is amply illustrated, and contains abundant accounts of the different forms of plants which may be used in accordance with the author's instruction. It is a work to which we give almost the highest praise.

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## A MANUAL OF COLOUR.*

THIS is an excellent little work, to which we wish all the success in the world. The author has gone on strictly scientific principles, and is perfectly correct in his ideas. We only wish that some one distinguished in the artistic world would take up the subject, and thus help in introducing Mr. Benson's general views on the subject of colour. Most assuredly whoever he was he would find himself a gainer and not a loser by the transaction, for he would then be able to master thoroughly the science of colour. He could tell at a moment the exact effect of one half-tint, whereas at present he can only depend upon experience, and if he has not this in each particular instance—if, in fact, he be not an artist of very considerable experience—he must remain in ignorance till he has tried. Now, if he had mastered Mr. Benson's scheme, all this difficulty would vanish, for he then would become possessed of a perfect scientific scheme for the estimation of colours, both simple and compound. We think very highly of the little book now before us.

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## SATURN'S RINGS.†

MR. DAVIES has written a work, which does him great credit, on the subject of Saturn's rings. He asks the question, Have those rings been always there, and if not, how have they come? Of course the problem involves some mathematics, and even with their aid cannot be determinately proven. Still the author thinks he has proved satisfactorily that the rings of Saturn are due to the attraction of the planet which has drawn into it, and is still drawing towards it, multitudes of meteors, which are gradually increasing its bulk. If it be asked, Why should one planet alone do this? he answers, because it is almost the farthest away from the sun; hence its attraction for these meteors is greater relatively than that of other planets much

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* "Manual of the Science of Colour, or the True Theory of the Colour Sensations and the Natural System." By William Benson, Architect. London: Chapman and Hall, 1871.

† "The Meteoric Theory of Saturn's Rings, considered with reference to the Solar Motion in Space." By A. M. Davies, B.A., F.R.A.S. London: Longmans, 1871.

nearer the sun. Indeed, this is the most powerful of all his arguments, and we confess that it by no means satisfies us. We are dissatisfied with the whole theory, which we think rests on too little foundation. Still others may not think so, and they will do well to consult the volume, which is well written and amply illustrated. It contains, too, a paper on the meteoric theory of the sun, which we have not said anything about.

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## BRITISH BUTTERFLIES.*

THE British Butterflies have found a good friend in Mr. Newman, who has given us a history of their lives—from *larva* to *imago*, their habits and their whereabouts—which is one of the most perfect things of the kind. And we are glad to read the author's statement that his work has attained, while in progress, a sale that is almost unattainable in English scientific works. Firstly, the work consists of a series of notices to the young who may be disposed to go butterfly-hunting. And in them we find the author's great experience, and we commend this part of his work to our readers. The next part deals with the subjects of anatomy, physiology, and embryology of the insects; and finally we come to the separate account of each species. This latter is admirably given. First comes a capital engraving, life-size, of the species; and then follows in order the life, history, time of appearance and locality, occupying from a page to a page and a half or two pages of a large quarto (or nearly so) volume. All this is done well, as we might expect from the author; it is clear, intelligible, and devoid of much of the rubbish which abounds in books of this kind generally. We had intended to have quoted some passages from the introduction, but we have not space to do so; therefore we must conclude our notice by expressing the hope that all who are interested in insects will make themselves acquainted with the volume.

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## CASSELL'S TECHNICAL EDUCATOR.†

THIS is considered as a work uniform in aim with the "Popular Educator," but having of course, as the name implies, a more special application. It is generally very fairly done, some of the authors being very well qualified for the task of instruction, while others are as certainly very little adapted to their task. We notice that a good many of the contributors are Irishmen. This we think is a new feature of an English work, and by no means a bad one. Those whose names are down from Ireland constitute about the best of the whole of Messrs. Cassell's authors. As regards the quality of the matter, it is passable, some of the articles being very good

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* "An Illustrated Natural History of British Butterflies." By Edward Newman, F.L.S., F.Z.S. The figures drawn by George Willis, and engraved by John Kirchner. London: William Tweedie, 1871.

† "The Technical Educator; an Encyclopædia of Technical Education." Vol. I. Cassell, Petter and Galpin, 1871.

and others equally bad. We shall not say which we think the worst. But altogether, we think Messrs. Cassell have not done well in their selection of authors, and consequently the result is not of the best description.

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## FRAGMENTS OF SCIENCE.*

PERHAPS of all scientific men in England, Professor Tyndall is the most eloquent, whether in speaking or writing. This is a fact which we think very few who have read any of his works, or who have heard him speak, will for a moment doubt. There may not be in his speech that careful accuracy which is so eminently characteristic of Professor Huxley, but assuredly there is eloquence. There is a fine and noble eloquence about the man which those who are familiar with his public speaking cannot but admire intensely. But there is in the reprinted papers before us as much of that eloquence as there is in the Professor's speech, so that we cannot but regard them, from this aspect alone, as taking no ordinary rank in English literature. And apart from any qualities of style they may possess, they have merits of the highest order as scientific memoirs. Of the contents of the volume it is difficult to say which chapter or essay is best, more readable, or more intensely interesting. A peculiarity of the work is, that it contains some valuable essays on miracles and on spirit-rapping, in both of which the author shows clearly and effectively how little is in the popular idea, and how necessary it is to look calmly and philosophically at every phenomenon. We wish we had space to give the author's words, the more especially on certain of the subjects he has taken up, but we have not. We will therefore close our very brief notice of a most valuable work by quoting its contents:—The Constitution of Nature; Thoughts on Prayer and Natural Law; Miracles and Special Providences; Matter and Force; Address to Students; Scope and Limit of Scientific Materialism; Scientific Use of the Imagination; on Radiation; Radiant Heat in relation to the Colour and Chemical Constitution of Bodies; Chemical Rays and Structure and Light of the Sky; Dust and Disease (with additions to the original lecture); Life and Letters of Faraday; an Elementary Lecture on Magnetism; and lastly, a few shorter articles.

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*Iron and Heat*, by James Armour, C.E. London: Lockwood, 1871.—Essentially a work for the engineer. This little volume appears to be well got up. It is amply illustrated, and contains a great deal of information regarding the subject on which it treats. The chapters on smelting, though not so far advanced as they might be, are nevertheless correct, and go very far into the subject.

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* "Fragments of Science for Unscientific People; a Series of Detached Essays, Lectures, and Reviews." By John Tyndall, LL.D., F.R.S. London: Longmans, 1871.

*The Science of Building*, by E. W. Tarn, M.A. London: Lockwood.—The author has attempted to introduce the student of architecture to a general outline of the scientific subjects connected with his profession. The author has avoided abstruse calculation, and thus has rendered his book intelligible to those students whose acquaintance with algebra and Euclid is of a limited character. Numerous woodcuts are scattered through the volume, which we think a good introduction to the science.

*The Use and Limit of the Imagination in Science*, by John Tyndall, LL.D., F.R.S. London: Longmans, 1870.—This is one of the author's brilliant discourses on a most difficult subject. It is already familiar to most of our readers, so that we need say no more about it, save that in this edition the author has collected together and gives a series of critiques of different papers pro and con, which are really interesting reading.

*A Monograph of the Alcedinidæ, or Family of Kingfishers*, by R. B. Sharpe, F.L.S. London: Published by the Author, 1871.—This work has occupied the author from the year 1866 till the present time. It is now completed, and with 121 coloured plates forms the finest work of the kind in existence. Those who are interested in this wonderful group should consult Mr. Sharpe's inestimable work.

# SCIENTIFIC SUMMARY.

## ASTRONOMY.

DEATH *of Sir John Herschel.*—At a ripe age, yet before he had attained the years of his father, the greatest astronomer of our day has passed from among us. The time has scarcely yet arrived for drawing a comparison between the elder and the younger Herschel, or determining whether astronomy is more indebted to the grand conceptions of the father, or to the more tutored philosophy of the son. At present what we must chiefly regard is the fact that these two great astronomers have accomplished between them the most wonderful series of researches which astronomy has yet known. The whole heavens gauged, thousands of double stars discovered and observed, and nine-tenths of all the known nebulæ placed upon our lists by these two labourers alone, such are some among the achievements which must be credited to the Herschels; while throughout the whole progress of the work the world has not known whether to wonder most at the untiring zeal and energy of these two workers, or at the grandeur of the conceptions by which they gave meaning and value to their observations.

In the case of the younger Herschel we have to admire, not merely labours in the field of astronomical research, but profound and valuable mathematical inquiries, chemical studies of extreme interest, and a power over the difficult problems associated with optical research in which he was matched by few of his contemporaries. Nor must we forget to point out how important have been the services which Sir John Herschel has rendered to science, in those masterly descriptions by means of which he has succeeded in imparting something of his own earnestness and zeal to those who have followed him as their guide and instructor.

But Sir John Herschel possessed other qualities which, though in a sense belonging rather to his personal character than to his position as a man of science, yet are so far related to the latter that we may be permitted to dwell upon them here. A singular sweetness of disposition, a readiness to yield attention to the thoughts and opinions of others even when most adverse to his own, and a perfect willingness to admit his own mistakes when (as all men will) he fell into error—such qualities as these may well be held up to the admiration of men of science in an age when we have too often occasion to admit the truth of what the Poet Laureate has sung, that

> The man of science himself is eager for glory and vain,
> His eye well practised in nature, his spirit bounded and poor.

The "mental purity" which Sir John Herschel advocated "as that quality which alone can fit us for a full and ready perception as well of moral beauty as of physical adaptation," is after all but another word for mental strength. The effort to loosen the hold on all which might interfere with the ready admission of truth is, as Herschel justly said, "the 'euphrasy and rue' with which we must 'purge our sight' before we can receive and contemplate as they are the lineaments of truth and nature:" but it is chiefly to be valued as indicative of strength of mind; while the seeming firmness of grasp with which some men retain their hold, with vice-like tenacity, upon favourite theories, is as far from showing real strength as the clutch of a convulsed person. It is well, then, to note how our greatest astronomer—like Newton and Sir William Herschel, the greatest astronomers of other days—was submissive to truth, patient to hear the reasoning of others, and ready to yield his own opinions when sufficient arguments were urged against them.*

*The Spectrum of Uranus.*—Dr. Huggins has already begun to do good work with the fine telescope placed in his hands by the Royal Society. With the 8-inch equatorial he employed before, he found the light of Uranus too faint to be satisfactorily examined with the spectroscope. But with the 15 inches of aperture of the refractor by Grubb, he has succeeded in studying the planet to some purpose. His results are not in accordance with those obtained by Fr. Secchi in 1869. Fr. Secchi then remarked of the spectrum of Uranus: "Le jaune y fait complètement défaut. Dans le vert et dans le bleu il y a deux raies très-larges et très-noires." The band in the blue he represented as less refrangible than F, and the one in the green as near E. On the contrary, Dr. Huggins describes the spectrum of Uranus as "continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from C to about G." The most marked absorption lines in the spectrum of Uranus are six. One of these (the most refrangible) is at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction spark, was compared directly with that of Uranus, and "the band in the planet's spectrum appeared to be coincident with the bright line of hydrogen." Two of the other bands "appeared to be very nearly coincident with bright lines of the spectrum of air, but the faintness of the planet's spectrum did not admit of certainty on the point." "I suspected," adds Dr. Huggins, "that the planetary lines are in a small degree less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely, the double line of nitrogen. As carbonic acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took mea-

* We saw, with pain, that in the most elaborate obituary notice of Herschel which appeared in the daily press—the biography in the "Daily News"—the most beautiful qualities of Herschel's disposition, those which specially marked the philosophic nature of his mind, were (most perversely) ascribed to vanity—a quality, of all others, most alien to his disposition. From internal evidence, we are inclined to believe that this notice, obviously written by one who was not a very young man in 1830, came from the pen of a certain eminent mathematician, who did not live to see it in print.

sures of the principal groups of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas. There is no absorption band at the position of the line of sodium." Further, "there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the Earth's atmosphere." The presence of hydrogen in the atmosphere of Uranus in quantities sufficient to produce recognisable absorption (if the coincidence above described be confirmed) must be regarded as a fact of singular interest and importance. The line is altogether too strong to be regarded as representing merely the Fraunhofer F line in the reflected solar light.

*The supposed Change of Colour of the Equatorial Belt of Jupiter.*—At the May meeting of the Royal Astronomical Society a discussion was raised upon this subject, and the general opinion of those who took part in the discussion—including Drs. Huggins and De La Rue, the President (Mr. Lassell), and others—was that no change had taken place of late in the colour of the planet. It was suggested that the colour of the equatorial belt is seen now by more observers than in former years, simply because more observers use silvered glass reflectors of large aperture. Mr. Browning, however, points out that this explanation will not hold good. "Five years ago," he says, "I began making careful coloured drawings of Jupiter, with a reflector 10½ inches in aperture. As long since as December 13, 1867, I drew attention to the fact that colour is best seen with small apertures or high powers. I worked with powers of from 350 to 500, when the air would permit. Although at that time I easily saw the coppery-grey of the dark belts, and the bluish-grey of the poles, I could detect no strong colour on the equatorial belt. Yet for the last two years the tawny colour of the equatorial belt has been more conspicuous than either. It is true that during the last three years I have had a 12¼-inch reflector, but, owing to unfavourable atmospheric conditions, practically I have seldom indeed used more than 10 inches of aperture. Several observers have also seen the tawny colour of the belt with both refractors and reflectors of only three or four inches' aperture. The exact colour of the equatorial belt may be obtained by allowing a very powerful light to shine through a jet of steam, so that an increase in the luminosity of the body of the planet would completely account for the colour of the belt." *Adhuc sub judice lis est.*

*Proposal for a double Automatic Spectroscope with Compound Prisms.*—Encouraged by the success with which Mr. Browning has carried out the plan devised by Mr. Proctor for a double automatic spectroscope (an instrument on this plan was exhibited at the last *soirée* of the Royal Society), Mr. Proctor has proposed a double battery with compound prisms, instead of the single prisms of the former. The mechanical contrivance differs in no essential respect from the former (slightly modified from the picture in *The Sun*), on which the double battery of single prisms was constructed; but the form of the intermediate prism is necessarily modified, since this prism as well as the rest has to be compound. The dispersion which would be given by this instrument would correspond to that given by thirty-six single equilateral prisms of heavy flint glass.

It is to be noticed that the plan by which the light is made to pass twice

through a single or double battery of prisms (single or compound) can readily be extended to cause the light to pass four, or even six or eight times through a battery. It is only necessary that the prisms, already doubled in height, that the light may pass twice through them, should be made four, six, or eight times as high as in an ordinary battery, rectangular prisms, precisely like the one cemented to the last half-prism in the return battery, being provided to raise the light rays, story by story, so to speak, as they pass backwards and forwards through a battery of these tall prisms. We believe Messrs. Grubb have proposed to make for Dr. Huggins' telescope a four-storied battery of compound prisms; while we learn that in America Professor Young and Dr. Langley are having instruments constructed on this plan.

We note here that Mr. Browning claims priority of Messrs. Grubb as respects the construction and employment of compound prisms. He remarks, that since July 1869, he "has made several compound prisms of very dense flint glass and light crown glass for the present Earl of Rosse." He is now constructing a compound prism formed of three crown and two flint prisms for Mr. Lewis Rutherford, of New York. It is not a direct vision prism, as might be supposed from this description, the two outermost crown prisms being of small refracting angle. Mr. Browning remarks that the reason he has not used compound prisms more frequently is that such prisms are more expensive than ordinary ones, even allowing for the extra dispersive power obtained, and that, "in consequence of the minimum angle of deviation of compound prisms being greater" (*sic*, but the word should obviously have been *less*), and their length greater, the size of spectroscopes would require to be increased, and they would thus be rendered more cumbersome as well as more expensive."

*The Total Eclipse of the Sun on December* 11, 1871.—Attention is already beginning to be directed to this eclipse. It seems unlikely that any expeditions will be sent out from England to observe it; but as the track of totality passes over parts of India, Ceylon, and Northern Australia, it is probable that useful observations will be made. In India especially it is likely that skilful observers may have an opportunity of studying the eclipse, since the northern boundary of the track of totality lies but a short distance south of Madras, where there is an excellent observatory, under the management of Mr. N. Pogson. The following account of the most important features of the eclipse is extracted from a paper by Mr. Ragoonathachary, communicated by Mr. Pogson to the Royal Astronomical Society:—"The central line of the eclipse will first meet the Earth's surface in the Arabian Sea, and, entering on the western coast of India, will pass right across one of the most important parts of Hindustan in a S.E. by E. direction. In this part of the peninsula the Sun will be about 20° above the horizon when totally obscured. The duration of totality will be two minutes and a quarter and the breadth of the shadow about 70 miles. On leaving the eastern coast of the Madras Presidency the central line will cross Palk's Straits, passing about 10 miles S.W. of the island Jaffnapatam, and over the northern part of Ceylon, where the small towns of Moeletivoe and Kokelay will lie near the central line; and also the well-known naval station of Trincomalee, which will be about 15 miles S.W. of the line. Con-

tinuing its course over the Bay of Bengal, the shadow will cross the S.E. part of Sumatra, and will touch the south-western coast of Java, where Batavia, the capital, will be nearly 60 miles N.E. of the central line; and two other smaller towns, Chidamar and Nagara, will also be very near the middle of the shadow-path. In the Admiralty Gulf on the N.W. coast of Australia, the eclipsed Sun will be only ten degrees past the meridian, and not far from the zenith; in consequence of which the totality will last 4m. 18s., or only 4 seconds less than the time of greatest duration. Lastly, passing through the most barren and uninhabitated portion of Australia, crossing the Gulf of Carpentaria and the York Peninsula, the shadow will ultimately leave the Earth's surface in the Pacific Ocean." "The general circumstances under which the eclipse will occur," (Mr. Ragoonathachary here refers to India) "are singularly and unusually favourable, the greater portion of the shadow-path being easily accessible, by means of the railway and good public roads; while a well-managed line of telegraph will afford facilities for that most incomparable means of fixing the longitude of the place of observation with regard to Madras. The favourite sanatorium of the Presidency, Ootacamund, will doubtless be selected by many persons as a convenient and familiar station from which to observe the eclipse, as also the hilly region of Wynaad, in the Malabar district, where numerous European gentlemen reside for the purpose of superintending their coffee-plantations. The lofty peak of Dodabetta, the highest point of the Neilgherries, 8,640 feet above the sea-level, would, agreeably to the oft-repeated and enlightened view of Professor C. Piazzi Smyth, the Astronomer Royal for Scotland, offer a grand opportunity for spectroscopic observations in an atmosphere of small density, and free from all the impurities which abound at lower levels, but unfortunately haze and mist are very prevalent on the hill-ranges in the month of December. The weather is in general fine elsewhere about that time along the shadow-path, but more especially so eastward of the Neilgherry hills than towards the Malabar coast."

*The Greenwich Observations of the last Solar Eclipse.*—Among the most important researches carried out during the eclipse of last December must be ranked the observations made at Greenwich, for the purpose of determining the error of Hansen's lunar tables at the period of conjunction. The plan of operations was similar to that arranged for the eclipse of 1860. Clouds interfered with a portion of the work, but the rest was very efficiently carried out. The observations were discussed (at the cost of no inconsiderable amount of labour) under the able superintendence of Mr. Dunkin. The following are the most important results:—

The Sun's tabular error was found to be—

$$\text{In R.A.} = +0''\cdot 11$$
$$\text{In N.P.D.} = +2''\cdot 2.$$

The Moon's tabular error was found to be—

$$\text{In R.A.} = +0''\cdot 54$$
$$\text{In N.P.D.} = +2''\cdot 54.$$

The error of the Moon's tabular geocentric semi-diameter $= 3''\cdot 48$.

The effect of the corrections on the choice of stations would be very small. The central line would be shifted nearly five miles farther south, and the breadth of the shadow increased by about double that amount, whilst the time of first contact would be accelerated and of last contact retarded by about 10s.

These last results may seem to be *now* unimportant; but it is well to notice how the observation of eclipses is preparing the way for increased accuracy, both as respects the prediction of future eclipses, and that analysis of the features of past eclipses which has already thrown so important a light not merely on astronomical questions but on historical events.

*The Solar Corona.*—Since our last summary appeared, more facts have come to light respecting the solar corona, as seen during the last eclipse, and there has been a good deal of discussion over the significance of the new facts brought to our knowledge. On nearly all sides the atmospheric glare theory of the corona has now been abandoned, though the repeated statement of this fact by various writers in the correspondence-columns of a contemporary weekly science-paper has given rise to as repeated editorial denials (the editor being the chief, we had almost said the only remaining, advocate of the glare theory in this country). But although the solar nature of the corona is now generally admitted, we are very far indeed from having solved the problems of difficulty presented by solar phenomena. Indeed we may be said to have barely entered upon this difficult field of research.

Various ideas which had been broached respecting the corona can now be discussed under somewhat more satisfactory circumstances than before the late eclipse. Two papers have appeared during the last two months which bear on the theoretical considerations suggested by the observations made last December. In one Mr. Proctor points to the significance of the observed connection between the corona, the prominences, and the solar spot regions; and he points out that, judging from the evidence now in our hands, the theory seems suggested that all the phenomena—corona, prominences, and sun-spots—are dependent "on the action of vertical forces, or, at any rate, of forces directed outwards from the Sun's globe—though not necessarily exactly radial." Abandoning the meteoric theory of the corona, or rather speaking of it as insufficient to account for the observed phenomena, he considers the probable effects of eruptive or repulsive forces exerted by the Sun on matter in the first place within his visible globe, and he shows that many somewhat perplexing phenomena seem to receive an interpretation when this theory is (provisionally) adopted. In the other paper, Professor Young, of America, after pointing out some objections to the meteoric theory of the corona, remarks that the low specific gravity of the coronal matter may depend on the action of "such solar repulsion as appears to be operative in the formation of a comet's tail." In this paper he points out very lucidly how small the influence must be which our atmosphere is capable of exerting in increasing the seeming importance of the corona. "Some influence our atmosphere must, of course, have; but remembering how much the inner portion of the coronal ring exceeds in brilliance the outer, it would seem that the illumination of the lunar disc must give us an exaggerated measure of the true atmospheric effect. This illumination makes the edge of the Moon

only enough brighter than the centre to give it the appearance of a globe, but of almost inky blackness." He remarks, "Mr. Lockyer, in 'Nature,' quotes from a letter of mine written nearly a year ago, to show that my opinions regarding the nature of the corona have been considerably modified since then; and this is true to a certain extent, though I think the present approximation of our views is owing quite as much to a change in his own ideas—as would be evident on referring to his papers of the same and even somewhat later date. But I should *still* write, 'I am strongly disposed to believe that the whole phenomenon (i.e. the corona as I saw it in 1869) is purely solar.'" In 1869 the atmosphere was far clearer, it is to be noticed, than during the eclipse of 1870.

*The Zodiacal Light.*—Mr. Birt records some interesting observations of the zodiacal light in the "Monthly Notices of the Astronomical Society" for April. They were made in the earlier months of the year 1850, and have remained hitherto (for unknown reasons) unpublished. Mr. Birt, by the way, seems unaware of the extensive series of observations of the zodiacal light made by German astronomers, for he remarks that so full and consecutive a series as his own has not, he believes, been published—a strange mistake. However, his observations are interesting in so far as they confirm those already made, especially as respects the change in the form of the "light" as seen at different seasons. He submits, as a new theory, the generally accepted explanation that these changes are due to the changing position of the observer as the Earth travels onwards on her orbit.

In connection with this subject we must refer to a most preposterous note which appears in the same number of the "Notices," in which an observer says, "We think we saw the zodiacal light on Good Friday evening. When we first observed it, it was, as nearly as I can tell (for my watch was slow) about 7h. 4m., and it lasted about five minutes. It was a little to the north of where the Sun had set behind a hill, and the ray of light was nearly perpendicular, and, as nearly as I could guess without means of measurement, about a degree broad and five degrees long." The observation is interesting enough in itself, but can have had no possible connection with the zodiacal light, whose appearance and position (at any season) should be too well known for such an account as the above to be referred to that phenomenon.

*Orbits of the Binary Stars $\zeta$ Herculis and $\zeta$ Cancri.*—Mr. W. E. Plummer, of Mr. Bishop's Observatory, Twickenham, has re-examined the observations made upon these two stars. By means of equations of condition founded on observations made on $\zeta$ Herculis from 1782 to 1869, he obtains the following elements:—

| | |
|---|---|
| Date of the periastron . . . . . | 1866·241 |
| Period of the revolution . . . . . | 36·606 years |
| Longitude of the node . . . . . | 27° 0′·4 |
| Longitude of the periastron . . . . | 291° 49′·0 |
| Inclination . . . . . . . . | 50° 14′·1 |
| Eccentricity $\epsilon = 0·55110$, $\phi =$ . . . . | 33° 26′·5 |
| Mean motion in minutes . . . . . | [2·77090] |
| Mean distance . . . . . . . | 1″·374. |

The observations available in the case of $\zeta$ *Cancri* extend over the last ninety years. The resulting elements are :—

| | |
|---|---|
| Periastron passage . . . . . . | 1072·44 |
| Period of revolution . . . . . . | 58·23 years |
| Longitude of the node . . . . | 15° 37′·4 |
| Longitude of the periastron . . . | 171° 46′·8 |
| Inclination . . . . . . . | 36° 14′·4 |
| Eccentricity $\epsilon = 0{\cdot}30230$, $\phi =$ . . . | 17° 35′·8 |
| Mean motion in minutes . . . . | [2·56930] |
| Mean distance . . . . . . . | 0″·908. |

*Winnecke's New Comet.*—This comet was first seen in Perseus, on April 8, at about half-past eight in the evening. From Dr. Winnecke's observations, combined with two made by Mr. Hind at Twickenham Observatory, the latter astronomer has deduced the following elements of the orbit :—

Epoch 1871, June 9·28137 G. M. T.

| | | |
|---|---|---|
| Longitude of perihelion . . | 146° 19′ 8″ | Referred to apparent equinox April 10. |
| Longitude of the ascending node . | 280° 53′ 32″ | |
| Inclination . . . . . . | 86° 50′ 54″ | |
| Log $q$ . . . . . . . | 0·7854883. | |

Motion direct.

Mr. Hind remarks that the comet appears to be quite distinct from any previously computed. Dr. Huggins has examined the comet spectroscopically, with results not differing from those he obtained from the study of other comets—the spectrum of the new comet presenting three bright bands.

*A comprehensive Star-Chart.*—Mr. Proctor is engaged in the construction of an isographic chart of the northern heavens, in which are to be included all the stars (324,000 in number) of Argelander's noble series of charts. Mr. Proctor's object in charting these stars on a single sheet is to endeavour to ascertain what laws of distribution exist among the stars of the first nine or ten orders of magnitude. Struve has already examined a portion of the same list of stars with a somewhat similar object; but as he dealt only with numerical relations, and these relating only to averages, it seems not unlikely that the presentation of all the 324,000 stars in a single view, all the details of their arrangement being preserved, may lead to results of extreme interest.

*Observations of Mars by Mr. Joynson.*—Mr. Joynson continues to send in to the Astronomical Society, after each opposition of Mars, an elaborate series of lithographs of the planet, as seen with his equatorial of about four inches in aperture. The drawings must cause Mr. Joynson immense labour, and doubtless his hope is to advance astronomical knowledge ; but one would like to have "a reason of the hope that is in him." Mr. Dawes, with a splendid 8-inch equatorial and unequalled power of vision, has given us more than enough drawings to form a complete chart of Mars, showing more, probably, than most observers could expect to see, under the most favourable circumstances, with a 10-inch refractor. What useful purpose can be subserved by labouring at a survey with about a fourth part of the power he

employed, and with certainly inferior observing skill? Let Mr. Joynson's own answer be taken. "These drawings," he says, "in conjunction with those previously sent, prove beyond doubt that the 'band' and the wine-glass-shaped channel' from it, are permanent features of the planet; and that any apparent change in them arises from the various aspects that are presented by the planet itself, as seen from the Earth"—facts which were ancient before Mr. Joynson was born.

*Comets which will be visible during the approaching Quarter.*—In the "Monthly Notices of the Astronomical Society" Mr. Hind gives elements of Tuttle's comet from September 1, and of Encke's comet from August 21. As we have already somewhat exceeded our usual space, we do not here quote these elements; it is the less necessary that we should do so, as doubtless all who are likely to study these two objects will receive (or have already received) copies of the elements from Mr. Hind himself.

*The Planets during the ensuing Quarter.*—Venus is now very favourably situated for observation, and will continue to be so for some time; she will attain her greatest brightness as an evening star in August. Neither Mars, Jupiter, nor Saturn will be well situated for observation during the next quarter.

## BOTANY.

*A Climbing Fern.*—This plant (*Lygodium palmatum*) exists and flourishes in its wild state within the borders of "old Essex," U.S. The writer in the "American Naturalist" discovered this rare and attractive plant in 1869, while exploring "Lynn Woods," in the vicinity of the famous "Penny Bridge." The locality of its haunt is within the limits of Saugus, and not far from that romantic spot known as the Pirates' Glen. Specimens have been obtained having a stalk or "vine" nearly four feet in length. "As the climbing fern is one of the most rare, graceful, and attractive plants found in this country, it is a matter of satisfaction to know that we have it growing in our woodland valleys." This fern has been found, though rarely, in Florida, Kentucky, and Massachussets. In Virginia it is often seen, and it has been found in several other localities.

*Botanical Gardens of Europe.*—It seems, from certain recently published statements regarding the time at which the several principal gardens were established, that the first one was that of Padua, in 1545, followed by that of Pisa. Those of Leyden and Leipsig date respectively 1577 and 1579. The Montpelier garden was founded in 1593; that of Giessen in 1605; of Strasburg, in 1620; of Alford, in 1625; and of Jena, in 1629. The Jardin des Plantes, at Paris, was established in 1626, and the Upsal garden in 1627; that of Madrid dates from 1763; and that of Coimbra from 1773. At the close of the eighteenth century, according to Gesner, more than 1,600 kindred establishments existed in Europe. England comes late upon the list, the Oxford garden not having been founded until 1632, and long remaining the only one in the kingdom.

*A Peculiar Ink-plant.*—The "Pall Mall Gazette" states that there is in New Granada a plant, *Coryaria thymifolia,* which might be dangerous to

our ink manufacturers if it could be acclimatised in Europe. It is known under the name of the ink plant. Its juice, called chanchi, can be used in writing without any previous preparation. The letters traced with it are of a reddish colour at first, but turn to a deep black in a few hours. This juice also spoils steel pens less than common ink. The qualities of the plant seem to have been discovered under the Spanish administration. Some writings, intended for the mother country, were wet through with sea-water on the voyage; while the papers written with common ink were almost illegible, those with the juice of that plant were quite unscathed. Orders were given in consequence that this vegetable ink was to be used for all public documents.

*Yeast and other Ferments.*—The "Journal of the Quekett Club" for April contains a useful paper by Mr. C. A. Watkins on this subject. The author rather endeavours to collect the information which exists, and to analyse it, than to put forward new ideas. This paper contains a vast amount of information, but there is little that pertains to satisfactory reasoning in it.

*Ascent of Sap in the Pines.*—A curious fact is pointed out by a writer in one of the American journals. He says, that some years ago his gardener pointed out to him that some Austrian and Scotch pines, which had been completely girdled by mice, still continued to grow, as if no such injury had been received. In order to test this matter, he took an Austrian pine about five feet high, and girdled it for a space of two inches, at about three feet from the ground. This was five years ago, and the upper portion is still alive. The tree attracts much attention from visitors to his grounds. When girdled, the branch was about one and a half inch in diameter. The whole portion of stem between the tier of branches above, and that below—a space of about fifteen inches—has since remained of that size, and is dry and hard as a "pine knot." The parts above and below this dead space increase annually in girth. The upper portion is now about nine inches in circumference. There are branches above and below the girdled portion; the lower ones growing much the stronger. The upper portion makes only two or three inches of growth a year, and the "needles" are of a brighter green than the lower.

*Contrivance in the Corolla of Salvia Involucrata.*—In most salvias part of the anther develops into a lever, which closes the throat, and, when lifted by an insect, causes the pollen to be thrown on its back. Some suppose, and with apparent good reason, that this is to aid in cross-fertilisation. In *Salvia involucrata*, according to Mr. Thomas Meehan, who writes to the "American Naturalist," the lever arrangements are remarkably well developed, but the arched upper lip curves inward, and prevents the anthers from acting in the manner above described. It would seem as if the plant, after "making" its arrangements for cross-fertilisation, "repented," and "made" another to contradict it.

*A New Fibre for the Manufacturer.*—A new fibre, obtained from the bark of the mulberry tree, has been produced by Mr. G. B. Marasi. It is expected that the new material will answer almost all the purposes for which hemp and flax are employed.

*New Species of Oaks from North-west America.*—Mr. R. Brown, M.A.,

President of the Physical Society of Edinburgh, has contributed a paper on this subject to the "Annals of Natural History" for April. He gives a minute account of five species, thus: *Quercus Sadleriana, Q. Œrstediana, Q. echinoides, Q. oblongifolia,* and *Q. Jacobi.* In all, seventeen species of Cupuliferæ find a place in the flora of the region to the west of the Rocky Mountains, northward of and including Upper California, which immense extent of territory, so varied in its climate and physical features, is generally known as North-west America. As he has already described and figured most of these species for a general work on the forests of that country (now in course of publication), he does not mention them in this place; and for the same reason he has omitted to give figures of the species he has here described, these figures, with more extended descriptions, being intended to find a place in the same work.

## CHEMISTRY.

*The Formation of Ozone by Resin Oils* has been the subject of a paper by Mr. C. R. C. Tichborne, which was read at a meeting of the Royal Irish Academy. The author said, when light resin oils were submitted to the combined action of atmospheric oxygen and light, ozone was produced in abundance. ("Such oils, when poured upon a solution of iodide of potassium, instantly produce blue iodide of starch.") At the same time the boiling-point of the oil was raised. Ozone was probably the prime mover in the production of colophonic hydrate, a substance discovered about two years ago by himself. All the terebine oils on exposure to light produced this effect. That obtained from pine seeds was said to possess this property in a most marked manner; oils of lemon and bergamot in a slight degree. The resin oils possessed this property more decidedly than ordinary oil of turpentine. Mr. Tichborne, by experiments, showed the action of those ozonified oils upon iodide of potassium.

*The Chemistry of Seed Germination* has been investigated by Herr Dr. Vogel of the Royal Bavarian Academy ("Proceedings," vol. ii. No. 3, 1870). A memoir published in the foregoing Proceedings, and which has been abstracted in the usual valuable columns of the "Chemical News," contains the detailed account of a series of experiments made by the author, with a view of ascertaining the effect which various substances (as, for instance, aniline, amorphous phosphorus, dilute solution of permanganate of potassa, sulphate of copper, dilute acetic acid, dilute hydrocyanic acid, arsenious acid, illuminating gas, naphthaline, and other substances have upon the germination of vegetable seeds. Among the substances which thoroughly impede, and even destroy, the power of germination, the author mentions dilute (0·21 per cent.) acetic acid and phenylic acid (1 drop to 50 c. c.) It appears that otherwise the germination is not much affected by very dilute aqueous solutions of most of the substances above quoted.

*Illumination by a Substance called Carboxygen.*—The "Journal für Gasbeleuchtung" (No. 8) contains a paper on this subject by Dr. J. Philips. The carbolin (the fluid used in the lamps invented by the author) costs about

the same as petroleum—that is to say about 6*d.* per Prussian quart (1·45 litres). The hourly consumption of carbolin, amounting, on an average, to 30 grms., will cost about one halfpenny. As regards the price of the oxygen gas (an artificial air containing from 50 to 75 per cent. of oxygen), it is stated that this may be prepared on the large scale at a cost of 15 centimes (2½*d.*) per cubic metre (a little more than 35 English cubic feet). The carboxygen illumination is daily gaining ground in Cologne, Brussels, New York, and other places. This light is also employed now in some large hot-houses where plants and shrubs are exhibited, the colours of leaves and flowers seen by this light being seen exactly as in daylight, while there is no danger of injuring the most tender and delicate plants, as there is with other artificial modes of lightning.

*Gustav Bischof in a Scotch Chair.*—We learn from the "Medical Press" that M. Gustav Bischof has been appointed to the "Young" chair of Technical Chemistry in the Andersonian University at Glasgow. M. Bischof is son of the late Professor of Chemistry at Bonn, and well known as the author of "Chemical Geology."

*Advantages of Infusorial Silica.*—The "Scientific American" recently had an article on this subject. It says, that by mixing three to six parts of infusorial silica to one part of freshly burnt lime, and stamping the whole, after slightly moistening it, into a suitable mould, artificial stone of any desired form can be made. Such stones become extremely hard, are impervious to water, are finer grained than cements of *béton,* can be used for gas or water pipes, and will take any colour. As there are large deposits of diatomaceous earth in various parts of America, this application for artificial stone and cement is well worthy of consideration. By combining infusorial earth with native magnesite and chloride of magnesium, a cement is produced which is known in Germany under the name of albolith cement. The chloride of magnesium, obtained as an incidental product in salt manufacture, is very cheap in some parts of Germany, and the occurrence of large deposits of magnesite renders this variety of cement available in Europe for many purposes. A fine glaze for earthenware is obtained by fusing infusorial earth with crude borate of lime, or boronatrocalcite. A variety of porcelain can be made by fusing infusorial silica with the borate of magnesia of the Stassfort mines. This kind of porcelain can be cast, pressed, and, if sufficiently thin, can be blown as easily as glass. It is capable of extensive use in the arts.

*Oxygen burnt with a Sooty Flame.*—The "Journal of the Franklin Institute" contains an account of this experiment by Professor Thomson of Copenhagen. It says that heavy hydrocarbons, like benzol and oil of turpentine, burn with a very sooty flame; with a very similar flame oxygen also burns in the vapours of these bodies. The experiment is made in the following manner:—Some benzol is warmed to the boiling-point in a long-necked flask; the flask is closed by a cork with two holes, through one of which a glass tube of about 1 centimetre bore is passed, and through the other a tube somewhat narrower and bent to one side. When the vapour arrives at the orifice of the wider tube it is lighted, and then a tube, through which a slow stream of oxygen is flowing, is passed down into the flask through the flame. The oxygen tube is bent above, and its mouth is pro-

vided with a platinum tube fused into it. A cork upon the oxygen tube closes the wider tube of the flask. The benzol flame is extinguished, and the vapour issues through the side tube, while the oxygen burns in the benzol vapour with a very sooty flame.

*A Manual of Organic Chemistry*, by Dr. Henry E. Armstrong, F.C.S., Professor of Chemistry in the London Institution, is advertised by Messrs. Longmans & Co., as being in preparation for their series of Text-books of Science.

*Oil from Grape-pips.*—The "Berichte der Deutschen Chemischen Gesellschaft," No. 8, contains a paper on this matter, by Herr S. Fitz. The pips contained in grapes contain from fifteen to eighteen per cent. of an oil, which has been the subject of a series of researches made by the author. The oil, having been saponified, was found to contain palmitic, stearic, and erucic acids, the latter fusing at 34°; formula, $C_{22}H_{42}O_2$. The lead-salt of this acid is difficultly soluble in ether in the cold, but readily so in warm ether; the same obtains with alcohol. The erucic acid exceeds in quantity the two other acids just named, the three being, in the neutral oil, combined with glycerine. When fused with caustic alkali, erucic acid is converted into arachnic acid, $C_2\,H_{40}O_9$, and into acetic acid. The author states that the oil alluded to might be used for culinary purposes; of course it is only obtainable in quantity in wine-growing countries.

*The British Association's next meeting* will be held at Edinburgh, and will commence on Wednesday, August 2, under the presidency of Sir William Thomson, M.A., LL.D., D.C.L., F.R.S., &c. The facilities now afforded by the several railway and steam-boat companies to parties travelling from all parts of Great Britain and the Continent, render it probable that this meeting will be very numerously attended. The local authorities and the representatives of the various scientific societies, as well as all those officially connected with the Association, earnestly desire that the members and Associates should receive a cordial welcome, and that everything possible should be done to make the visit agreeable and instructive.

*Chloroform from Chloral and Chloroform from Alcohol and Bleaching Powder.*—These two varieties of chloroform it is important to distinguish, and therefore the following tests, which are quoted in the "Chemical News," from "Douglass Journal" for May, are of importance:—In the first place, the former chloroform, known now on the Continent as "English chloroform," has a sp. g. of only 1·485, and contains, according to the author, from 0·75 to 0·8 per cent. of strong alcohol. When, to the chloroform prepared from alcohol and bleaching powder, strong sulphuric acid is added, it always becomes more or less coloured, which does not happen to be the case with the chloroform prepared from chloral; when, moreover, a few drops of each kind of chloroform are left to evaporate spontaneously upon a watch-glass, the chloroform prepared with alcohol gives off, after a while, a disagreeable smell, while the other kind retains until complete volatilisation its pleasant fruity odour.

*Excrements of the common Bat Rhinolophus Hipposiderus.*—In the April number of the "Annalen der Chemie," Professor Wöhler, after briefly referring to the researches on the excrements of the Egyptian bat by Dr. Popp, states that Dr. Ehlers had the kindness to supply him with the

material above named, taken from a locality where it had accumulated to a layer of 3 inches' thickness. This substance does not contain any trace of urea, nor any uric or oxalic acids; the bulk is made up of the undigested horny mass of the wings of insects. Dried at 100°, this substance gave 8·25 per cent. of nitrogen, and left, after ignition, 6·25 per cent. of ash, containing potassa, soda, lime, magnesia, oxide of iron, chlorine, sulphuric, silicic, and phosphoric acids, the latter being 36 per cent. of the weight of the ash.

*The Crystalline Substance covering the Vanilla Pod.*—M. P. Carles has contributed an article to an early number of the "Journal de Pharmacie" on this subject. The article treats on the nature of the crystalline substance which may frequently be seen on the vanilla pods, and have been considered by various authors to consist of benzoic or cinnamic acid, or coumarine. The author finds that this crystalline matter fuses at about 81°; boils, with partial decomposition, at 280°; is very readily soluble in alcohol, ether, chloroform, and sulphide of carbon; difficultly soluble in cold water; exhibits a strong acid reaction to blue litmus paper; and decomposes carbonates. Elementary organic composition, $C_{16}H_8O_6$. The author has prepared several salts of this acid; but, as to its true chemical composition, his labours are not yet completed. See also "Chemical News."

*Ozone.*—At the meeting of the Chemical Society on June 1, Dr. Debus, F.R.S., delivered a lecture on the subject of ozone. The lecturer began by stating why ozone is considered to be an allotropic modification of oxygen; then discussed whether there are reasons to assume the existence of two allotropic modifications, and concluded with a review of some of the properties of ozone.

*Himalaya Tea.*—In the "Annalen der Chemie" for May we find a paper by Dr. P. Zöller. This paper contains the results of a series of experiments made with a sample of very fine tea, a small quantity of which had been sent to Professor J. Von Liebig by a proprietor of tea plantations. 100 parts of the tea contained 4·25 of water and 5·93 of ash, which, in 100 parts, was found to consist of—potassa, 39·22; soda, 0·65; magnesia, 6·47; lime, 4·24; peroxide of iron, 4·38; manganoso-manganic oxide, 1·03; phosphoric acid, 14·55; sulphuric acid, a trace; chlorine, 0·81; silica, 4·35; carbonic acid, 24·30. Extract soluble in water of this tea amounted to 36·26 per cent. The nitrogen in the air-dried tea was found to be 5·38 per cent. In addition to 4·94 per cent. of theine, this tea was found to contain some theobromine. The quantity of nitrogen in the extract (that is, the portion of the tea soluble in water by infusion), dried at 100°, is 10·09 per cent., while the ash therein amounts to 11·46 per cent. The author concludes by stating that this tea is equal to the best Chinese tea, and that the younger the leaves of the shrub the better the tea they yield.

*Formation of Transparent Cubes of Chloride of Sodium similar to Rock-Salt.*—The "Chemical News" gives in its usual list of abstracts one, of a paper by Dr. Buchner in the "Journal für prakt. Chemie" (No. 6, 1871), on the above subject. This paper is, in a measure, the reproduction of the contents of an essay published by Dr. Mohr (Poggendorff's "Annalen," vol. cxxxv., p. 667, 1868), "On the Formation of Rock-salt." The author relates, in confirmation of the facts adduced by Dr. Mohr, a few

instances which prove that the formation of cubes of rock-salt and of other chlorides isomorphous therewith, actually takes place slowly by the spontaneous evaporation of concentrated brines and the gradual diffusion of the thus supersaturated solution downwards (in the vessel wherein that solution is contained), so that the cubes form, and increase gradually in size, at the bottom of the vessel which contains the saline liquor. The author also observed, in one instance, the formation of chloride of ammonium in cubical crystals deposited from the so-called Kœchlin's copper liquor (*Liquor cupri ammoniato-muriatici*), the evaporation having been caused by the not well fitting of the stopper; but this process had taken years to produce the cubical form of crystals of sal-ammoniac alluded to.

*Certain Fatty Ketones.*—In a paper read before the Royal Society, April 2, Dr. J. Emerson Reynolds gives a long account of the above. He draws the following conclusions. 1. That certain fatty ketones can be made to unite directly with mercuric oxide. 2. That the resulting compounds afford a new group of colloid hydrates, analogous in properties to the silicic, aluminic, and other hydrates already made known by the researches of Professor Graham. 3. That the new hydrates may best be regarded as extremely feeble conjugate acids; the chief member of the group (that derivable from acetone) being probably tetrabasic and capable of affording very unstable salts. 4. That the generating reaction for these bodies may, when carefully controlled, be employed as a test for the presence of a ketone, especially for acetone in certain organic mixtures. 5. That the generating reaction for the di-keto-mercurates may, when carefully controlled, be employed as a test for the presence of a ketone—especially for acetone—in certain organic mixtures.

*Beet-root Sugar in Ireland.*—The Rev. J. Jellet recently (May 29) read a paper before the Royal Irish Academy on the above subject. Six specimens of the white beet, grown at the Model Farm at Glasnevin, had been examined with the Jellet saccharometer. The sliced root was digested with weak alcohol three or four times; the solutions were evaporated and then made up, after having been heated to 180°, to a given bulk with water. This solution was filtered through charcoal, and the first portions rejected as a certain amount of the sugar is absorbed. The sugar present in the beet almost entirely consisted of cane sugar. The following are the results of the six determinations :—

| | | | |
|---|---|---|---|
| 1st. | 12·05 per cent. | 4th. | 12·59 per cent. |
| 2nd. | 9·50 „ „ | 5th. | 11·62 „ „ |
| 3rd. | 12·58 „ „ | 6th. | 12·43 „ „ |

Professor Jellet said that anything over 10 per cent. might be considered very good, and would pay to work.

## GEOLOGY AND PALÆONTOLOGY.

*The Glacial Drift of Massachusetts.*—Prof. N. S. Shaler gives, in the "Proceedings of the Boston Society of Natural History" (vol. xiii.), a very able account of the parallel ridges in the above. In the immediate

neighbourhood of Boston the unstratified drift does not lie in anything like a continuous sheet, but is distributed in long and rather narrow ridges, which, with varying height, on account of long-continued denudation, may be traced for miles across the country. These ridges are particularly conspicuous in the islands of the harbour of Boston, where, although much worn by the action of tidal currents, the parallelism is quite apparent. They exhibit two sets of trends, the one being north-west and south-east, the other north-east and south-west; and a comparison of the sections, given at various points in the islands of the harbour, at Chelsea, Somerville, Cambridge, Brighton, South Boston, and elsewhere, has shown that throughout this region the drift is remarkably similar at the same height above the sea. The drift contains pebbles of various sizes, five-foot boulders, and fragments of every gradation down to coarse sand. The whole is embedded in a fine mud, which so binds the materials together that, in the lower parts of the mass, where it has been subjected to considerable pressures, it has become a hard conglomerate. Nowhere is there any attempt at stratification. In regard to the origin of these drifts, Professor Shaler agrees with Agassiz in considering them as the materials which rested in and upon the glacial sheet at the close of its history, and which were dropped in the places they now occupy by the melting of the ice upon which they rested. As this drift deposit must have originally been at least one hundred and fifty feet thick, it is difficult to conceive how such a mass of detritus as that in question could have ever been contained in a glacial stream, and the supposition is necessary that the mass of the drift must have been rent from the floor of the glacier as it moved along.

*A new Fossil Myriapod* is described by Mr. Woodward, F.G.S., in the "Geological Magazine" for March. It was discovered by the late Mr. Thomas Brown, after whom he names it. The specimen, preserved in the round, is four inches in length, and nearly a quarter of an inch in breadth, is contained in an ironstone-nodule (like those previously discovered), both sides of which are well preserved. Thirty-six raised body rings, separated by an equal number of intermediate depressions, can be counted between the head and the last segment, each ring having two pairs of appendages. There are indications of pores, and also of the bases of tubercles or spines, along the dorsal line, but the latter are less perfectly preserved. In their great work on the "Geological Survey of Illinois," 1868, vol. iii. p. 556, Figs. A–D, &c., Messrs. Meek and Worthen have described and figured two new forms of myriapod under the generic name of *Euphoberia*—namely, *E. armigera* and *E.? major*. The specimen described is named *E. Brownii.*

*The Skeleton of Dinornis.*—Professor Owen read before the Zoological Society, at its meeting on June 6, a paper on Dinornis, being the seventeenth of his series of communications on these extinct birds. The present paper gave a description of the sternum and pelvis, and an attempted restoration of the whole skeleton of *Aptornis defossor.*

*Life of Mr. Davidson.*—The "Geological Magazine" for April gives a very interesting sketch of the life of Mr. Davidson, the well-known authority on Brachiopoda. It gives also an excellent portrait of this distinguished man, and a list of his writings, occupying two pages of small type. We may also mention that at the anniversary meeting of the

Palæontographical Society (March 31) this year, the council of that society presented to Mr. Davidson a copy of his magnificent work on British Fossil Brachiopoda, handsomely bound, as a small expression of their high estimation of his valuable and protracted labours for the promotion of the objects of the Society.

*The Wollaston Medal and Fund.*—The Wollaston medal has this year been presented to Professor Ramsay, who acknowledged the compliment in a very humble address. Professor Ramsay well deserves the medal, and ought, we think, to have got it earlier. The proceeds of the Wollaston Fund were awarded to Mr. Etheridge, to enable him to prosecute his valuable work on the Fossils of the British Islands. Mr. Etheridge has been on this work for eight years, and the work itself occupies nine volumes of manuscript. We hope this splendid work may be soon completed and offered to the public.

*The Palæontographical Society* has issued its yearly volume for 1870. It contains several most important works, and among them one by Professor Owen, who concludes the volume with a monograph on the Fossil Mammals of the Mesozoic Rocks, the materials for which he has long been accumulating, and the interest in which has by no means abated. The first pages of this work are devoted to a consideration of the Rhætic Mammals of the genus *Microlestes*, in which the author places the detached tooth of *Hypsiprimnopsis rhæticus*, discovered by Mr. Boyd-Dawkins at Watchet, Somerset, and also the remarkably rich series of detached teeth discovered by Mr. Charles Moore, F.G.S., of Bath, in a fissure of the Mountain Limestone at Holwell, Frome, Somersetshire. Then follow the Mammalia from the Stonesfield Slate of the genera *Amphitherium*, *Phascolotherium*, and *Stereognathus*, comprising four species. The remainder of the work is occupied by the consideration and description of the Purbeck Mammalia, which (with the exception of *Spalacotheriuum tricuspidens*, Owen discovered in 1854 by Messrs. Wilcox and Brodie, of Swanage) were all brought to light through explorations carried on with characteristic ardour, and at much cost and personal risk, by Samuel H. Beckles, Esq., F.R.S. They occupy (with woodcuts) ninety-three pages of letter-press and nearly the whole of four plates. The descriptions of these early types of small Marsupial Mammals are founded almost entirely upon the evidence afforded by lower jaws, not more than half-a-dozen specimens being found in which the upper dental series are preserved, and no crania are as yet known. Above ten genera and twenty-five species have been determined from the Purbeck beds, Durdlestone Bay, Dorsetshire, and described by Professor Owen. The plates have been most carefully and successfully drawn by Mr. Alfred T. Hollick.

*Death of Sir John Herschell.*—One of our greatest philosophers has gone from among us. Though, of course, more of an astronomer than a geologist, he was nevertheless a distinguished thinker in matters of geologic science. He died on May 11 last, at the advanced age of seventy-nine, in the full possession of all his mental faculties. Though he devoted most of his time to astronomy, natural philosophy, chemistry, meteorology, physical geography, etc., geology did not altogether escape his attention. Among his suggestive contributions to this science may be mentioned the following:

—1. On Changes of Climate arising from the varying Excentricity of the Earth's Orbit ("Geol. Trans.," 2nd series, vol. iii., referred to in "Lyell's Principles" as early as 1837). 2. On the effect of the Removal of Matter from above to below the Sea, producing "a mechanical subversion of the equilibrium of pressure and temperature;" On Subsidence and Elevation; The Influence of Subterranean Steam; The results of the Expansion of Rocks by Heat; The Fusion and Metamorphism of Sedimentary Rocks, etc. (letters written in 1836, and published in 1838, at the close of "Babbage's Ninth Bridgewater Treatise"). He was buried in Westminster Abbey, beside the remains of Sir Isaac Newton.

*The Pre-Glacial History of North Cheshire.*—Mr. C. E. de Rance, who publishes a somewhat lengthy paper on the geology of Cheshire in the "Geological Magazine," thinks, among other conclusions, that in pre-glacial times a plain of marine denudation composed of hard and soft beds of new red sandstone existed from the borders of Wales to south-western Lancashire, unbroken by valleys, over which flowed the Dee to the north, receiving as a tributary from the east the Mersey, which gradually cut for itself a transverse gorge across the strike of the rocks; at the same time, longitudinal valleys, to the north and to the south, gradually came into existence. Those to the north were afterwards entirely destroyed by marine denudation, which formed a lower plain. The subsidence continuing and the climate becoming glacial, the district was submerged beneath the waters of the Glacial Sea, and the Gorge, or transverse valley, as well as the longitudinal valleys, were filled up with glacial deposits. Afterwards, on the re-elevation of the country, these were excavated out, partly by running water, and partly perhaps by small glaciers which, as he has attempted to show elsewhere, undoubtedly held their ground, at the close of the glacial epoch, in the valleys of the Lake-district. The entire valley of the Mersey, including its termination through the Wallasey Gorge, would be equally filled up with drift; over this surface of drift the river must have flowed, widening and deepening its channel as it ran, here making great cliffs of overhanging boulder-clay, and there cutting through the drift down to the bare rock, and in some instances cutting its bed wider and deeper in the rock than it was before the glacial submergence.

*Mr. Hennessey and M. Delaunay* versus *Mr. Hopkins.*—On the 13th of last March, just as we were going to press, M. Delaunay read a statement to the effect that he acknowledged that Mr. Hennessey had used the same arguments as himself against Mr. Hopkins's theory relative to the fluidity of the interior parts of the earth. This admission, which is a most honourable one, must have been received in Dublin with great rejoicing, and very worthily so.

*Sea-sand Heaps caused by Glaciers.*—Mr. J. H. Kinham proposes a somewhat startling but, nevertheless, very well sustained hypothesis on this subject. Since accumulations of sand occur at or in the vicinity of all the valleys of Yar-connaught that open towards the west; and as in each of them there is palpable evidence that glaciers once flowed down them towards the west, he cannot but be inclined to believe that these sands originally owed their origin to glaciers. That similar accumulations do not always exist at or near the mouths of the valleys in that country, which

open eastward, seems due to their glaciers being feeders of the glacier of the Lough Corrib valley, which itself was a branch of the glacier that flowed down the valley now occupied by the waters of Galway Bay; however, at the mouth of some of these valleys they do exist, and are described in the "Memoirs of the Geological Survey of Ireland" (sheets 95 and 105). Furthermore, his belief is strengthened when he considers that all the accumulations of this kind of sand in Ireland, with which he is intimately acquainted, both at or near the sea-board, and inland, have similar relations to valleys in which, if he has not observed the traces of glaciers, yet it is not only possible, but also highly probable, that they once existed. Since the glacial period, on account of the loose and frail nature of the Æolian sand, they have been a prey to the caprice of the wind or other moving forces, and have been drifted hither and thither, and their real relations to the more recent deposits have been obliterated.

*Organic Remains in the Crags.*—An important list of these is given in a paper by R. Bell, in the "Geological Magazine" for June. The following is a list of the nett total of each group of which the author has lists:—

| Summary. | Lower crag. | Middle. | Upper Red (marine). | Norwich Fluvio-marine. | Pre-glacial. |
|---|---|---|---|---|---|
| Cetacea | 2 | 21 | — | — | 3 |
| Other mammalia | 1 | 14 | — | 6 | 23 |
| Aves | — | — | — | 1 | — |
| Pisces | 9 | 3 | 2 | 2 | 5 |
| Insecta | — | — | — | — | 1 |
| Crustacea | 9 | 2 | 1 | — | — |
| Ostracoda | 21 | 4 | — | — | — |
| Cirripedia | 10 | 8 | 3 | 3 | 3 |
| Annelida | 4 | 1 | 2 | — | 1 |
| Echinodermata | 17 | 11 | 2 | — | 3 |
| Land and fresh water mollusca | — | 5 | 9 | 22 | 19 |
| Marine Gasteropoda and Solenoconcha | 193 | 178 | 108 | 64 | 46 |
| Opisthobranchiata | 14 | 5 | 3 | 4 | 3 |
| Pteropoda | 1 | — | — | — | — |
| Lamellibranchiata | 169 | 135 | 74 | 71 | 73 |
| Brachiopoda | 5 | 1 | 2 | 2 | — |
| Polyzoa | 125 | 30 | 5 | — | 3 |
| Cœlenterata | 4 | 5 | 2 | — | — |
| Protozoa | 1 | 2 | — | — | — |
| Rhizopoda | 88 | 26 | — | 10 | 5 |
| Plantæ | 2 | 1 | — | — | 12 |
| Total in each formation | 675 | 452 | 213 | 185 | 200 |

*American Survey of Iron and Copper Mines.*—From the "Report on the Progress of the State Geological Survey of Michigan," by A. Winchell, L.L.D., we learn that the survey of the iron region near Marquette is nearly completed. Eleven large maps of the most important mines are nearly ready

for the engraver. Discoveries have been made of new and large beds of iron ore in the forest unsettled country, upon lands owned by the State. The older beds belong to the Huronian system, several thousand feet thick. All the rocks appear to have been of sedimentary origin, though often presenting combinations suggestive of an igneous character. The following is their order, in descending scale:—1. Quartzite; 2. Hematic and Magnetic Ores; 3. Ferruginous Quartzite; 4. Diorite; 5. Ferruginous Quartzite; 6. Diorite; 7. Ferruginous Quartzite; 8. Diorite; 9. Ferruginous Quartzite; 10. Diorite; 11. Talcose Schist; 12. Quartzite; 13. Laurentian. The copper region, under the superintendence of Professor Pumpelly, is being mapped upon the scale of 300 feet to the inch. The fieldwork has led to the accumulation of numerous details respecting the distribution of the several formations, which cannot be presented in a rêport of progress, but they have necessitated many improvements upon the Geological Map.

*Quality of Ancient Limestone for Building Purposes.*—In the "Bulletin de l'Académie Royale de Belgique," No. 2, 1871, M. Omalins d'Halloy gives a paper of some importance on this subject. The main view of the author is that it is not so much the texture of the limestones which has to be taken into consideration, since the structure and texture of these stones may vary immensely, and yet they may all be suited for building purposes, provided the layers or beds have not been, as very frequently is the case, dislocated by geological upheavings, whereby many of these kinds of stone become foliated, and do not then withstand wind and weather for any length of time without crumbling to pieces.

*A New Chimæroid Fish from the Lyme Regis Lias.*—On April 5 last Sir Philip Grey Egerton, Bart., read a paper on the above. This fish, for which the author proposed the name of *Ischyodus orthorhinus*, was represented by a specimen showing the anterior structures embedded in a slab of lias. It exhibited the characteristic dental apparatus the Chimæroids, surrounded with shagreen, a very large prelabial appendage six inches long, and terminating in a hook abruptly turned downwards, and a process which the author regarded as representing the well-known rostral appendage of the male Chimæroid, but in this case attaining a length of 5½ inches, and covered more or less thickly with tubercles, bearing recurved central spines somewhat tooth-like in their aspect. This appendage is attached to the head by a rounded condyle, received into a hollow in the frontal cartilage. The dorsal spine, which measured 6 inches in length, was articulated by a rounded surface to a strong cartilaginous plate projecting upwards from the notochordal axis, and was thus rendered capable of a considerable amount of motion in a vertical plane. This structure also occurred in *Callorhynchus* and *Chimæra*.

*Approximation of the Crinoidea to the Tunicata.*—This strange relationship has been imagined by Mr. J. Rofe, F.R.S., who, in a paper published in the "Geological Magazine" for June, supports his views rather ably. He says, "In some other respects there appears to be an approximation of some of the Crinoidea to some of the Tunicata, as in the pyramidal valvulse of Cystidea and the Chelyosoma; and the outer tough bags of some of the Tunicata also contain radiated concretions sometimes siliceous, but more frequently calcareous, thus approaching the test of the Echinodermata. If

also, as is supposed, the Crinoids received their nourishment and the water necessary for respiration through the arms and the covered channels connected with them by an internal mouth, probably the power of expansion and contraction above alluded to may have been used by the Crinoids for the purpose of rapidly ejecting water to clear the internal passages, as is done by the *Ascidium* or sea-squirt, and by the mollusca generally. No doubt many objections may be raised to this supposed approach of two different and perhaps distinct orders; we know, however, that nature draws no hard lines, but abounds in connecting links, and it may be possible that the Molluscoidea and the Echinodermata are connected by the link now suggested.

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## MECHANICAL SCIENCE.

*Strains in Ships.*—Mr. E. J. Reed, C.B., has communicated to the Royal Society a paper containing the results of the remarkable and elaborate investigations of the strains in ships of various types, in which he was engaged before leaving the office of Chief Constructor. The enormous labour involved in such calculations has hitherto prevented the attainment of definite and precise results, even for still-water strains. Mr. Reed has carried out the necessary calculations for several types of vessel, and moreover has estimated the straining action due to the position of a ship on a wave-crest and in a wave-hollow. The importance of these calculations, as affecting the choice of one type of vessel rather than another, may be judged from the following comparison of the straining action in the *Minotaur* and *Bellerophon*:—

**On Wave-Crest.**

| | "Minotaur." | "Bellerophon." |
|---|---|---|
| Maximum shearing strain . . . | 1,365 tons. | 555 tons. |
| Maximum bending movement | 140,300 foot tons. | 43,000 foot tons. |

**In Wave-Hollow.**

| | | |
|---|---|---|
| Maximum shearing strain . . . | 695 tons. | 640 tons. |
| Maximum bending movement | 78,800 foot tons. | 48,800 foot tons. |

It may be useful to note that a brief abstract of this paper appeared in the "Mechanics' Magazine" for May 5.

*The "Devastation" and the "Cyclops."*—The Committee on Designs for Ships of War have made a report to the Admiralty respecting these vessels. The result at which they have arrived is, that "ships of the *Devastation* class have stability amply sufficient to make them safe against the rolling or heaving action of the waves;" and in addition, "as the steadying effects of bilge keels and of friction have been neglected in their calculations, the errors are on the safe side." They estimate that the wind might produce an angle of heel of five degrees in addition to that due to the roll of the waves, and that with this the ship would still have an ample margin of stability. They recommend that the *Devastation* should be completed with

the superstructure, and the *Thunderer* without it, and suggest certain minor alterations in the designs. In regard to the *Cyclops* class of vessels, they are equally satisfied as to their stability, even without a superstructure, under any conditions of wind and sea to which they will be likely to be exposed in the defence of our coast and in making voyages from port to port in favourable weather. They recommend that the four vessels of this class in course of construction should be completed with certain minor modifications.

*Stability of Ships.*—Some advance is made in the theory of the stability of ships, which since the disaster to the *Captain* has assumed so great a practical importance, in an interesting paper, read before the Institute of Naval Architects, by Mr. W. H. White and Mr. W. John. The paper is too mathematical for extract in these notes, but will well repay perusal.

*Machine for Testing Metals.*—An extremely interesting machine for testing metals on a new method has been invented by Mr. G. Bischoff, of Bonn, and was described in a paper read before the British Association. It has since been exhibited at the Institute of Naval Architects, and illustrated in engineering. Mr. Bischoff first prepares small test strips of the metal whose quality is to be ascertained. These are 7 mm. and 65 mm. long, and are prepared specially by methods which need not here be described. The test strips are then placed in a machine called a metallometer, in which the test strips are bent backwards and forwards through a definite angle, by preference an angle of 67½°. These bendings are effected by a clockwork arrangement, and indicating dials are provided to register the number of oscillations to which each strip is subjected. Ten strips can, if necessary, be tested at one time. The number of bendings which each strip sustains is, on Mr. Bischoff's system, the measure of the quality of the metal, and, according to his experiments, would seem to be an exceedingly delicate test. In order to have some fixed standard to which to refer the tests of other specimens, Mr. Bischoff selects strips of chemically pure zinc. The resistance of such strips is remarkably uniform. Knowing the average test of say 50 strips of zinc, in any given machine at any given angle of bending, we have a standard with which to compare the results of tests on other materials in the same or other machines. So delicate does Mr. Bischoff believe his method of testing to be, that he asserts he can by its aid detect the deteriorating effect of ·00,001 per cent. of tin when alloyed with pure zinc. The objection to Mr. Bischoff's system is that at all events, in certain cases, the preparation of the test strips involves processes which alter the mechanical properties of the material.

*New Form of Braced Bridge.*—Professor Fleeming Jenkin has invented a new form of braced arch bridge, and has described the mode of determining the stresses on the different parts of it, in a paper communicated to the Scottish Society of Arts. The stresses are determined by a new graphic method of great interest in itself, and which is due to Professor Clerk Maxwell. Professor Jenkin's braced arch is something like an ordinary Warren girder, in which the bottom boom, instead of being straight and parallel to the top boom, is curved into an arch. In the common bowstring girder the depth of the girder is greatest at the centre of the span and least at the ends. In Professor Jenkin's, on the other hand, the depth is greatest

at the ends and least at the centre of the span. In the top boom or member of the braced arch the distribution of material is similar to that in the boom of an ordinary girder. But in the bottom or arched member the section is greatest at the ends and least at the centre of the span. Mr. Jenkin calculates that a braced arch of 500 feet span, of the same strength as the Britannia Bridge, would weigh only 0·65 ton per foot run, whereas the girders of the Britannia Bridge weighed 3·8 tons per foot run. He states that there would be no difficulty in making the braced arch of cast iron in simple pipes, and that the cost of the superstructure of such a bridge would be only one-sixth that of a wrought-iron girder bridge. The braced arch is also suitable for construction in timber.

*Compound Engines.*—In a paper on the Compound Engines fitted in H.M.S. *Briton*, by Mr. G. B. Rennie, it was stated that the ordinary consumption of fuel had proved not to exceed 2 lbs. per independent high pressure per hour. Supposing a ship to carry a given quantity of coal and to steam at a given rate, with a given expenditure of power, Mr. Rennie has calculated the following table of the relative length of time required to empty the coal bunkers, with ships fitted with engines of different types:—

| Type of Engine. | Coal consumed per Ind. H. P. per Hour. | Number of Days' Steaming with 240 Tons of Coal. |
|---|---|---|
| 1. Improved Compound Engine | 2 | 5 days 14 hours. |
| 2. Ordinary Engine with superheaters and surface condensers | 3½ | 3 " 4 " |
| 3. Ordinary Injection Engine | 4½ | 2 " 11 " |
| 4. High Pressure Engine | 6 | 1 " 21 " |

Nothing could exhibit more clearly the importance of economical types of engine for steam navigation, and especially for heavily armoured ships of war. The actual consumption of fuel on the trial trips of the *Briton* varied from 1·515 to 1·981 lbs. per independent high pressure per hour. That on the trial trips of the *Tenedos*, with compound engines built by Messrs. John Elder & Sons, varied from 1·35 to 2·32 lbs. per independent high pressure per hour.

## MEDICAL.

*Edinburgh University Philosophical Society.*—The first session of the Edinburgh University Philosophical Society closed on Wednesday, April 5, Principal Sir Alexander Grant, Bart., delivering the valedictory address, the greater part of which consisted of an able criticism of the Darwinian theory of the descent of man.

*Influence of Non-nitrogenous Food on Man.*—Dr. Parkes has been carrying on his inquiries into the subject of the influence of foods of different kinds on man. In the *Proceedings of the Royal Society* (March) he gives a long paper, in which we find it stated what is the influence of a non-nitrogenous diet on the system. For instance, the circulation was materially influenced. Dr. Parkes says that with an equal pressure the lever was thrown

almost double the height when the man was on nitrogenous food. This feebleness of expansion shown by the sphygmograph was quite in accordance with the impression given to the finger. The softness of the pulse proved it was not owing to increased resistance of the arterial wall. With regard to the temperature, the means are so close to those of the days on ordinary diet, that having regard to the fact that the period was shorter and therefore more liable to error, and that some observations were omitted on the marching-day, it may be concluded that a non-nitrogenous diet continued for five days neither raised nor lowered the temperature of the axilla and rectum. It therefore shows that when the nitrogenous food of a healthy man was reduced to one-half for five days, and he was kept for five days more without nitrogen, he was able on the fourth day after such deprivation to do a very hard day's work. The non-nitrogenous diet, consisting of butter, oil, starch, and sugar, kept him perfectly well; all functions seemed natural, the temperature of the body was unaltered, the pulse became very soft, and the sphygmographic tracings showed very feeble markings; but it was not materially altered in frequency. The circulation appeared to be properly carried on, as far as could be judged of by the man's own feelings. The health, as judged of by the man's feelings and the absence of objective signs, was perfect. On account, however, of the feebleness of the heart's action, it was not thought right to continue the experiments, which had, he believes, sufficiently proved that force necessary for great muscular work can be obtained by the muscles from fat and starch, though changes in the nitrogenous constituents of the muscles also go on, which have as one effect an increased though not excessive elimination of nitrogen after the cessation of the work.

*Bromide of Potassium in Poisoning by Strychnine.*—Dr. Herbert contributes a paper on this subject to the "New York Medical Journal" (March). He gave it in a case of poisoning by strychnia as a *dernier ressort*, in doses of ninety grains or more, every half hour. "In twenty minutes after the administration of the first dose there was perceptible improvement, which continued. In two hours the patient could move his arms. The bromide was then given at the rate of one drachm every hour; but, the convulsions coming on again with greater severity, the remedy was given for one hour every fifteen minutes. At the end of that time he felt easier again, and the bromide was continued in smaller doses, at intervals of a half-hour to two hours, according to circumstances, during the day and following night. In thirty-six hours from the time that the bromide was first given he was walking about, feeling a little weak, and occasionally a slight twitch. Concerning this case there are several important points that it would be well to note:—1. The length of time that elapsed before the effect of the poison was manifest. 2. A very marked tolerance of opium. 3. Vomiting afforded great relief. 4. The antidotal power of bromide of potassium. The naked facts only are presented; my professional brethren may draw their own inferences."

*The Gastric and Intestinal Tubules.*—These structures, in connection with their pathological relations, have been very carefully studied by Dr. Austin Flint, Professor of the Practice of Medicine in the Bellevue Hospital Medical College, U.S. We merely call attention to the memoir published

in the "New York Medical Journal" for March. It is a paper of some considerable length, and deals fully with the researches of Handfield, Jones, Fox and Fenwick.

*Electro-Puncture in Aneurism of the Aorta.*—Recent numbers of the "Gazetta Medica Italiana-Lombardia" contain four additional cases related by Drs. De Cristoforis and Machiavelli, in which electro-acupuncture was employed for the relief of aneurism of the arch of the aorta. They are related in considerable detail, and are of great interest, showing, at least, that the practice is an inoffensive one, and gives the patient relief from great suffering.

*Death of a Distinguished Obstetrician.*—Professor Pietro Luzzati, of Milan, died suddenly on March 22. He was one of the most accomplished obstetricians of his country.

*London Water.*—In his report for 1870 to the Registrar-General, Professor Frankland states ["Medical Press"] that during the year each person in the metropolis was supplied with twenty gallons of water. His remarks on the quality of water are generally favourable, but then he considers 1870 a favourable year to the companies, because of its extreme drought. Professor Frankland supplies also a statement of the weight of nitrogen contained in the organic matters found in each sample of water. Organic matters of animal origin are more highly nitrogenous than those of vegetable origin, and therefore the presence of any considerable proportion of organic nitrogen in river waters known, like those of the Thames and Lea, to be polluted by sewage, must be regarded as throwing grave suspicion upon their quality. Another table is given showing the amount of previous sewage or animal contamination. So far as chemical analysis can show, the whole of this can be oxydised and converted into mineral and innocuous compounds when the analyses were made; but there is always a risk that some portion may have escaped this decomposition, and produce disease in those who drink the water. The risk is much greater when the water is from rivers and shallow wells than when it is from deep wells and springs. Professor Frankland states that while the evidence of this previous contamination in the Thames and Lea waters exposes them to grave suspicion, he regards the same evidence (though greater in amount) in the Kent Company's water as practically of no importance, if access of drainage from the upper strata be rigidly excluded from their deep chalk wells, and since the spring of 1868 his analyses afford no indication of any such soakage into these wells. The whole report is more favourable than we imagine the next one is likely to be, the drought having greatly benefited the companies in the one respect of purity of their water.

*Detection of Picrotoxin in Beer.*—The "Food Journal" for April quotes from the Berlin "Wochenschrift" some important remarks on this subject. It gives a method of detecting the presence of picrotoxin, the poisonous principle of the seeds of *Cocculus indicus,* in adulterated beer. The analysis is based upon the fact that a solution of sugar of lead containing ammonia precipitates sugar, dextrin, gum, &c., but not picrotoxin, which, however, can be extracted by shaking up the acidulous solution with ether. The beer under examination is first treated with ammonia until the smell of that substance strongly manifests itself. The precipitate is allowed to settle. To

the liquid, as soon as it becomes clear again, an alcoholic solution of sugar of lead is added until another precipitate is formed, consisting of insoluble dextrin, sugar, &c. Sulphuretted hydrogen is added next, and the filtered liquid is boiled down. The acidulous residue is shaken up with ether and left to settle, when the ether containing the picrotoxin is drawn off. Picrotoxin is a strong tetanic poison, producing vertigo, convulsions, and death. It dissolves in strong sulphuric acid, forming a saffron-coloured solution. With sulphuric acid and potassic bichromate it assumes a red-brown, and, on heating, a dark brown colour. Another test for picrotoxin is given in its action on cupric oxide, which it reduces from alkaline solutions, and when boiled with dilute acids takes up water, forming a substance which also reduces cupric oxide.

*The Effect of Iodate of Potassium on Animals.*—In a recently published paper, read before the Royal Academy of Belgium, Dr. Meslen, the author, gave the record of some experiments upon dogs, to which this salt was given along with their food. He concludes from his experiments that iodate of potassium is a violent poison; but this paper is only a preliminary notice, it being the author's intention to publish an exhaustive account of his experiments, and of the effects of this salt on the blood and internal organs of the animals experimented with.

*Lardaceous Disease.*—At the retiring meeting of the Pathological Society for the present session, the report of the committee on Lardaceous disease was produced. This is the name recommended for adoption in the case that has received other names also. An increase of cholesterine and of chloride of sodium is said to be present, while the affected organs are deficient in potash. It was long ago pointed out by Dr. Dickenson that deficiency of alkali was probably dependent on its removal from the blood by profuse suppuration, after which the disease is most frequently met with.

*A Gift to Owen's College, Manchester.*—Miss Brackenbury, of Manchester, has signified her intention, according to the "Society of Arts Journal," to give 10,000*l.* for the establishment of a medical school in connection with the College, being 5,000*l.* for the erection of suitable buildings, and 5,000*l.* by way of endowment for the support of the department. It is suggested that, as the father of Miss Brackenbury was in the medical profession, it would be a graceful recognition for the governors to endow a Brackenbury professor.

*A New Theory of Nervous Action.*—This, which was put forward some time since by Dr. Robert M'Donnel, and escaped our notice, we now call attention to, for it is very remarkable, and worthy of serious consideration. The paper was read before the Royal Irish Academy. The theory is thus stated by the author:—"I conceive that the various peripheral expansions of sensitive nerves take up undulations or vibrations, and couvert them into waves capable of being propagated along nervous tissue (neuricity, as it has been named). Thus, the same nerve tubule may be able to transmit along it vibrations differing in character, and hence giving rise to different sensations; and, consequently, the same nerve tubule may, in its normal condition, transmit the wave which produces the idea of simple contact, or that which produces the idea of heat—or, again, the same nerve tubles in the optic nerve which propagate the undulations of red may also propagate, in normal vision

those which excite the idea of yellow or blue, and so for other senses. I advocate this undulatory theory of sensation in preference to the theory of distinct conductors—1st, because it is simple. 2ndly, because it is strongly supported by analogy, when compared with wave propagations in other departments of science. 3rdly. Because it appears to be in harmony with a large number of recognised physiological facts, which seem inexplicable upon the theory of distinct conductors."

*A New Opthalmoscope* has been devised by M. le Dr. E. Javal. The mirror is a plate of glass covered by a thin layer of platinum, and the lenses which serve to correct the refraction of the patient or the observer are replaced by a small Galilean telescope. This, by a simple mechanism, is made to act as an optometer, and is exact as well as convenient. A greater magnifying power is obtained by this contrivance than by ordinary instruments, while the remark is made that the instrument is capable of improvement in some details. It is a great desideratum to be able to correct hypermetropia and myopia, in all degrees, without the trouble of changing the glasses, and to gain a greater amplification of the fundus, and we hope this new opthalmoscope may be perfected.

*A National Collection of Surgical Instruments.*—Sir William Fergusson has proposed to form a national collection of surgical instruments, to be placed in the Museum of the College of Surgeons, London, to illustrate as far as possible the progress of surgical art in Great Britain, and the improvements made from time to time in surgical appliances and instruments.

*The Anatomy of the Ciliary Muscle.*—Dr. A. Iwanoff has contributed a very valuable paper on this subject to the "Archiv für Ophthal." (Bd. xv. Abth. iii. s. 284–298) which is very ably abstracted in the "New York Medical Journal" for March. In addition to his former researches into the anatomy of the ciliary muscle, contained in a previous number of the "Archives," Dr. Iwanoff has undertaken to discover what differences it may have in myopic and hypermetropic eyes. That the difference of refraction involves a great difference in the accommodative function has long been maintained, and a corresponding variety in the ciliary muscle is to be expected. It has been maintained that in hypermetropia the muscle would be found large and greater in bulk, while in myopia it would be thin and smaller. The examination revealed quite another state of facts. In twelve myopic eyes, whose axes were from 28 to 34 mm. in length, in all myopia being over one-quarter, there was no atrophy of the muscle, but it was thicker and longer than in emmetropia. The muscle is composed of two sets of fibres—one external and running in the meridians of the globe, pointed out by Bowman and Brücke; the other set internal, at the anterior part circular in direction, and described by Arlt and H. Müller. In myopic eyes the circular fibres were almost entirely wanting, and the meridional fibres unusually numerous. In four hypermetropic eyes, whose axes were from 19 to 20 mm., the ciliary muscle was found thin and pushed forward, while in myopia it was thick and shoved backward. In hypermetropia, the posterior portion of the muscle was atrophied, the anterior part hypertrophied; that is, the circular fibres were in excess. The difference, then, in the structure of the muscle in myopia and hypermetropia is that in myopia the meridional fibres are most numerous, in hypermetropia the circular most numerous.

## MICROSCOPY.

*Contents of the Microscopical Journal.*—During the past three months the contents of this journal, besides several letters, and the reports on the progress of microscopical science, have been as follows :—" On the Structure of the Podura Scale, and certain other Test-objects, and of their Representation by Photomicrography." By Lieut.-Col. Dr. J. J. Woodward, U.S. Army.—" Microscopical Examination of Water for Domestic Use." By James Bell, F.C.S.—" On the Winter Habits of the Rotatoria." By Charles Cubitt, C.E., F.R.M.S.—" The Magnifying Power of the Microscope." By Count Castracane.—" A Few Experiments bearing on Spontaneous Generation." By Metcalfe Johnson, M.R.C.S.E., Lancaster.—" On the Mode of Working out the Morphology of the Skull." By W. Kitchen Parker, F.R.S., President R.M.S.—" Linear Projection considered in its Application to the Delineation of Objects under Microscopic Observation." By Charles Cubitt, C.E., F.R.M.S.—" Optical Appearances of Cut Lines in Glass." By Henry J. Slack, F.G.S., Secretary R.M.S.—" Object-glasses and their Definition." By F. H. Wenham, Vice-President R.M.S.—" Transmutation of Form in certain Protozoa." By Metcalfe Johnson, M.R.C.S.E., Lancaster.—" Microscopical Examination of Two Minerals." By Prof. A. M. Edwards.—" Additional Observations concerning the Podura Scale." By Dr. J. J. Woodward, U.S. Army.—" Remarks on the General and Particular Construction of the Scales of some of the Lepidoptera, as bearing on the Structure of the 'Test Scale' of Lepidocyrtus curvicollis." By R. L. Maddox, M.D.—" On the so-called Suckers of Dytiscus and the Pulvilli of Insects." By B. T. Lowne, M.R.C.S.

*Photomicrographs for the Stereoscope.*—In the " Microscopical Journal" for May there is an abstract of Dr. R. H. Ward's interesting researches on this point. Dr. Ward informs us, that in order to photograph, without delay, any field of view which a working microscopist deems worthy of preservation, he should have a camera mounted on a plank which is blocked at one end for the feet of the stand used as a " working instrument." Then, whenever desired, the eye-piece is removed, the instrument levelled into a horizontal position and placed accurately on the plank, and the magnified image instantly thrown upon the focussing plate of the camera. Finding the usual band, passing around pulleys and over the fine-adjustment wheel, to be a slight annoyance in carrying out this plan with the stand he ordinarily uses (a large stand of the " Jackson " model), he makes the fine-adjustment by a somewhat soft cylinder of india-rubber lying upon the wheel. This cylinder is rather more than three inches long, is an inch and a half in diameter, and weighs about four ounces. It is open through its centre, like a tube with thick walls and small bore, and is mounted upon one end of a straight, light, wooden rod, the other end of which is supported on or near the top of the camera. It is prevented from rolling off from the fine-adjustment wheel by a horizontal wire, transverse to the axis of the apparatus, attached by a hinge-joint to a post at the side of the wheel, loss of motion is simply impossible, and an extremely fine and manageable motion is secured. The unequalled facility and cer-

tainty with which this apparatus can be instantly laid upon the fine-adjustment wheel, or turned back from it, is sufficiently evident.

*Importance of the Microscope in working at the Skull.*—Dr. Kitchen Parker, F.R.S., the President of the Microscopical Society, recently read a paper on the above subject before the Society. He says it is all microscopic work; step by step everything has to be made out by the most delicate microscopical manipulation, and but for that instrument the whole matter must have been kept secret to the end of the world. It is impossible to overrate the value of such means that lead to such researches, for now we begin to see the absolute Unity of the Vertebrate Series—to say nothing of the other primary groups of animals. The highest type—the human—passes through every stage of morphological structure seen in the series beneath: it does not stop at these stages; it does not utilise, so to say, the incipient structures that are ready to be so used, but runs rapidly along its own line, choosing, as it were, and refusing, until at length the perfect man is attained. Yet this perfection of parts, this production of a creature who in his lowest attributes is the "paragon of animals," is not brought about irrelatively to the rest of creation; it is merely *an elected consummation* of all that is highest and best in morphological structure.

*Bleaching Diatomaceæ.*—The following note has been published by no less an authority than Dr. Maddox. From some experiments conducted lately in bleaching both marine and fresh-water forms, the use of chlorate of potash and hydrochloric acid appears to him as likely to prove *very* useful in the examination, not only of the skeleton, but the disposition of the endochrome; and it seems probable the same may be employed with success on the minute Algæ and minute forms of organic life commonly accompanying them in the collection. The proportions used have been varied, but about 40 grains of the crushed chlorate to 1½ drachm of the acid in 1 ounce of water, placed in a 2 or 3 ounce phial, closed with a waxed cork, may be taken as the average proportions: the action of the evolved chlorine commences after a short period, and can be watched or modified as required. The use of this mixture for the examination of chitinous structures in insects was advocated by Dr. Hicks; but Dr. Maddox is not aware of its having been extended to the Diatomaceæ; therefore the suggestion is made in the hope it may be found useful by those engaged in the examination of the organisation of these very interesting objects.

## PHYSICS.

*How to fix the Iron-filings acted on by Magnetism.*—All our readers are aware how beautifully iron-filings arrange themselves when placed under the influence of the magnet. It is not such a simple matter to preserve these forms, and the following method, suggested by Dr. S. M. Mayer, in an article in the "Chemical News," June 9, seems an excellent one. It is as follows:—A clean plate of thin glass is coated with a firm film of shellac, by flowing over it a solution of this substance in alcohol, in the same manner as a photographic plate is coated with collodion. After the plate has

remained a day or two in a dry atmosphere, it is placed over the magnet, or magnets, with its ends resting on slips of wood, so that the under surface of the plate just touches the magnet. Fine iron-filings, produced by "draw-filing" Norway iron which has been repeatedly annealed, are now sifted uniformly over the film of lac by means of a fine sieve. The spectrum is then produced on vibrating the plate, by letting fall vertically upon it, at different points, a light piece of copper wire. The plate is now cautiously lifted vertically off the magnet and placed on the end of a cylinder of pasteboard, which serves as a support in bringing it quite close to the under surface of a cast-iron plate (1 foot in diameter, ½ inch thick), which has been heated over a large Bunsen-flame. Thus the shellac is uniformly heated, and the iron-filings, absorbing the radiation, sink into the softened film and are "fixed." He generally allows the heat to act until the metallic lustre of the filings has disappeared, by sinking into the shellac, and the film appears quite transparent. This degree of action is necessary when photographic prints are to be made from the plate; but when they are to be used as lantern-slides, he does not carry the heating so far. After the plate has cooled, it is allowed to fall upon its ends on a table, so that any filings which have not adhered may be removed.

*Seven Years' Magnetic Observations.*—The Rev. J. J. Perry has published, in a paper lately read before the Royal Society, the result of seven years' observations at Stonyhurst. He states that the yearly mean values of the horizontal force are found to vary progressively from 3·5926 to 3·6178 in British units, the mean for October 1, 1866, being 3·6034, with a secular acceleration of 0·0042. Calculating from the monthly tables the mean value of the horizontal force for the six months from April to September, and for the semi-annual period from October to March, we find the former to be 0·0005 in excess over the latter, showing that this component of the intensity is greater during the summer than during the winter months. Treating the dip observations in a precisely similar way, we obtain 69° 45′ 21″ as the mean value of this element for October 1, 1866, subject to a secular diminution of 1′ 49″·2; the extreme yearly means being 69° 48′ 47″ and 69° 37′ 52″. The resulting excess of 10″ for the winter months in the computed semi-annual means is so small that the observations tend mainly to show that the effect of the sun's position is not clearly manifested by any decided variation in the dip. Deducing the intensity from the above elements, we obtain for the summer months the value 10·4136, whilst that for the winter months is 10·4128. The intensity of the earth's magnetic force would thus appear to increase with the sun's distance, but the difference is not large enough to have more than a negative weight in the question under discussion. This weight, moreover, is lessened by the slight uncertainties arising from the probable disturbing causes at the first magnetic station.

*Attraction caused by Vibrations of the Air.*—In the "Philosophical Magazine" is a paper by Professor Challis, in which the author maintains that the condensation in waves propagated from a centre will vary inversely as the distance, and that the rate of diminution of the condensation or rarefaction with distance from the centre will be continually changed from the law of the inverse square of the distance to that of the simple inverse of the

distance, provided there be alternate condensations and rarefactions, as seems to be inevitable; for it is contrary to known hydrodynamical laws to suppose the possibility of a solitary wave of condensation. The above-mentioned velocity gives rise to a continual flow from the rarified into the condensed parts, and just in the proportion required for altering the law of diminution with the distance from the inverse square to the simple inverse. Professor Challis believes that the attraction of magnetism is caused by vibration, to which he might have added the attraction of gravity—a doctrine long since propounded by Robert Hooke, and of which an account is given in his posthumous works. In the revolving grate erected by Boulton and Watt beneath a steam-boiler at the Bank of England, the coal was fed by a scoop moved by a cam, which advanced the scoop gradually over an orifice, carrying coal with it, and then suddenly drew back the scoop, when the coal, by its inertia, remaining behind it, fell into the fire. In this case we have a backward and forward motion causing bodies subjected to it to travel in a certain direction; and if we suppose a similar motion to exist in the particles of bodies, an attraction like that of gravity will be the result.—*Society of Arts Journal.*

*Physics of Arctic Ice.*—Mr. Brown has published, in the "Quarterly Journal of the Geological Society" (February 1871), a very long and important paper on the above subject, especially as regards Scottish geology. The author concludes:—

($\alpha$.) That after the Tertiary period the country was covered with a great depth of snow and ice, very much as in Greenland at the present day; but possibly some of the mountain-tops appeared as islands. During this and the subsequent period glaciers ploughed their way down from the inland ice, and icebergs broke off and reached the sea through the glens, then ice-fjords.

($\beta$.) After this the country sank gradually, as Greenland is now sinking, to the depth of several hundred feet; and during this period most of the glacial laminated fossiliferous clays were formed. During this period boulders were deposited from the icebergs broken off from the glaciers of Scotland, as well as from the icebergs and other floating ice drifted both from the north and south, as was also the case during the former ($\alpha$) period.

($\gamma$.) The country seems then to have emerged from the water, but no doubt slowly, until the glaciers finally left the country.

($\delta$.) By this time the country was much higher than now, and the land being connected with the continent, the bulk of the present flora and fauna crept into it from various quarters, though the alpine plants still kept possession of the higher mountain-regions during a great portion of this epoch.

($\epsilon$.) A depression now took place, and the estuarini beds, or *carses*, of the Scotch rivers were formed. Much of the fossiliferous boulder-clay was formed as he has described it; is now under the sea; off the coast remains of its fauna are continually dredged up. Man had also by this time got into the country.

($\zeta$.) The land after this seems to have risen, in all probability, to its present level, for we have no certain evidence that since the dawn of history there were any oscillations of level.

*A Temperary Accidental Fountain.*—M. H. Vogelsang gives, in Poggendorff's "Annalen" (No. 2, 1811), a long account of a curious fountain

which came within his experience at Delft, in Holland. It appears that, in order to obtain fresh water for domestic purposes, a so-called Norton pump-tube was being rammed down in the alluvial soil (peat-bog chiefly). When at a depth of 17·5 metres below the surface, and very nearly as much below sea-level at that locality, the ram-block was suddenly lifted by a mass of gas issuing from the opening of the iron tube, immediately followed by water, which rose to a height of 14 metres, and continued to do so for some fourteen hours; after that lapse of time, the fountain became for a few weeks intermittent. The temperature of the water was 13°. The composition of the gas, in 100 parts, by bulk, was found, by Dr. A. C. Oudemans, jun., to consist of 91·2 of marsh gas and 8·2 of carbonic acid, with traces only of air, while carbonic oxide and heavy hydrocarbons were proved to be absent. The phenomena alluded to ceased when, after some weeks, the iron pump-tube was forced down to a depth of 25 metres.

*Iron as a Filter and Deodoriser.*—Attention has been called to the use of spongy iron as a deodorising material which, Dr. Voelcker considers of greater potency than animal charcoal. Sewage water passed through a filter of this substance is completely purified, and this filtered water, after having been kept six months protected from the air, was perfectly sweet, and free from any fungus growth. The spongy iron is obtained by calcining a finely divided iron ore with charcoal. Mr. Spencer, whose name is connected with the discovery of the electrotype, has for some time been advocating the use of a filter of this description. Its power of rendering water beautifully transparent, and apparently free from all organic matter, is its strong recommendation. We ourselves have had a considerable experience of the Spencer filter, and we consider it an admirable one.

*A Drop of Water on a Hot Plate.*—The "Society of Arts Journal" (May 26), quoting from Poggendorff's "Annalen," gives an account of an experiment made by E. Budde, to ascertain whether a Leidenfrost's drop with water could be produced at a less temperature than 100 C. The experiment was made by letting a drop of water fall upon a hot plate covered by a partially exhausted receiver, and it was found that the drop assumed the spheroidal condition at a temperature of 85 C., confirming the doctrine that the force which supports the drop obeys the laws of the pressure of vapours. The star shape assumed by the drop Berger was explained to be a phenomenon of vibration. If the drop is large it behaves like other vibrating bodies, and divides into aliquot parts, forming nodes or loops.

*Depolarisation of Iron Ships.*—Mr. Charles F. Henwood, a member of the Institution of Naval Architects, read a paper at the late meeting of the Institution (March 30) on the invention of the late Evan Hopkins, C.E., F.G.S., by which the sub-permanent magnetism of an iron ship, the principal cause of the deviation and consequent depreciation of the compass, is stated to be completely and permanently removed. The paper contained a number of extracts, and a table showing the deviations of the compass of H.M.S. *Northumberland* before and after depolarisation, which will be given in the "Transactions" of the Institute.

*A Colour Experiment in Optics.*—In a late number of the "Proceedings of the Royal Society" Sir Charles Wheatstone says that few experiments in physical optics are so beautiful and striking as the elegant pictures formed

by cementing laminæ of selenite of different thicknesses (varying from $\frac{1}{2000}$ to $\frac{1}{50}$ of an inch) between two plates of glass. Invisible under ordinary circumstances, they exhibit, when examined in the usual polarising-apparatus, the most brilliant colours, which are complementary to each other in the two rectangular positions of the analyser. Regarded in the instrument which he has described, the appearances are still more beautiful; for, instead of a single transition, each colour in the picture is successively replaced by every other colour. In preparing such pictures it is necessary to pay attention to the direction of the principal section of each lamina when different pieces of the same thicknesses are to be combined together to form a surface having the same uniform tint; otherwise in the intermediate transitions the colours will be irregularly disposed.

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## ZOOLOGY AND COMPARATIVE ANATOMY.

*Ceratodus a Genus of Ganoid Fishes.*—Dr. Albert Guthrie has sent a paper to the Royal Society descriptive of this curious fish, which has lately been discovered in Australia. It seems to be a species of ganoid fish, not very unlike the well-known Lepidosiren. After describing the various points of its anatomy in detail, the author of this important paper draws the following conclusions. 1. That *Ceratodus* and *Lepidosiren* (*Protopterus*) are more nearly allied to each other than to any third living fish, the latter genus diverging more towards the Amphibians than the former. 2. That the difference in the arrangement of the valves of the bulbus arteriosus cannot longer be considered of sufficient importance to distinguish the *Dipnoi* as a sub-class from the *Ganoidei*; but that the *Dipnoi* may be retained as a sub-order of *Ganoidei*. 3. That the sub-order *Dipnoi* may be characterised as Ganoids with the nostrils within the mouth, with paddles supported by an axial skeleton, with lungs and gills and notochordal skeleton, and without branchiostegals. 4. That a comparison of *Teleostei*, *Chondropterygii*, and *Ganoidei* shows that the two latter divisions, hitherto regarded as sub-classes, are much more nearly allied to each other than to the *Teleostei*, which were developed in much more recent epochs; and therefore that they should be united into one sub-class—*Palæichthyes*—characterised thus: heart with a contractile bulbus arteriosus; intestine with a spiral valve; optic nerves non-decussating.

*The Sub-axial Arches in Man.*—These form the subject of a very elaborate paper presented to the Royal Society by Mr. G. W. Callender. The author appears, so far as we can see, to admit Owen's division of the cranial vertebræ, but imagines that two or more are essential parts. This paper shows considerable knowledge of the subject, but, so far as we can see, it is open to very serious objections. The paper will be found in abstract in the "Proceedings of the Royal Society" (March), and will be read with interest.

*Experiments in Pangenesis.*—Mr. F. Galton, F.R.S., has undertaken a series of experiments which he thinks render it impossible to accept Mr. Darwin's doctrine of pangenesis. These consisted in breeding from rabbits of a pure variety, into whose circulation blood taken from other varieties had

previously been largely transferred. The results were the same as if no bleeding had taken place at all. Hence Captain Galton cries out against the doctrine of pangenesis. But it seems to us perfectly clear that no other result would have been expected, even by a Darwinian disciple, from Mr. Galton's experiments. We fail to see the force of his reasoning.

*The Process of Silicification of Animals.*—On June 2 a paper was read before the Geologists' Association by Mr. M. H. Johnson, F.G.S., on the above subject, and it contained some interesting facts. The author pointed out how a crop of sponges invested with their gelatinous flesh or sarcode, and living at the bottom of a deep ocean, were suddenly buried in a thick stratum of white mud, consisting of the minute shells of *Foraminiferæ*, that they then died, and while in the process of decomposition this interchange of materials took place—the nascent carbonic acid parting with its carbon in exchange for the silicon of the silicate of soda which sea-water is known to contain. At the close of the paper, the author produced a tadpole, upon which he had experimented, and which he had that afternoon subjected for two hours and a half to the action of nitric acid, without its undergoing any alteration, the inference being irresistible that the animal had become invested with a film of silica of sufficient thickness to protect it from the acid; another tadpole that had not undergone the same preparation having been converted into a brown cloud by immersion in the acid for the same time.

*Development of Isotoma Walkeri.*—In the second volume of "Memoirs" which the Peabody Academy has just published, there appears a most valuable paper by Mr. A. S. Packard, jun., on the above subject. Speaking of the head, the author says, that it is so entirely different from what it is in the adult, that certain points demand our attention. It is evident that at this period the development of the insect has gone on in all important particulars much as in other insects, especially the *Neuropterous Mystacides* as described by Zaddach. The head is longer vertically than horizontally, the frontal or clypeal region is broad, and greater in extent than the epicranio-occipital region. The antennæ are inserted high up on the head, next the ocelli, falling down over the clypeal region. The clypeus, however, is merged with the epicranium, and the usual suture between them does not appear distinctly in after life, though its place is seen, in the elaborate figures which accompany the paper, to be indicated by a slight indentation. The labrum is distinctly defined by a well-marked suture, and forms a squarish knob-like protuberance, and in size is quite large compared to the clypeus. From this time begins the process of degradation, when the insect assumes its Thysanurous characters, which consist in an approach to the form of the Myriapodous head, the front or clypeal region being reduced to a minimum, and the antennæ and eyes brought in closer proximity to the mouth than in any other insects. That other most essential Thysanurous characteristic, the spring, is now fully formed. It arises as a thick tubercle from the sternite of the penultimate segment of the abdomen, and subdivides into a pair of two-jointed finger-shaped prolongations. The tip of the abdomen is deeply bilobate, the median line of the body being deeply impressed.

*Australian Silk.*—The Acclimatisation Society of Sydney have received

some silk-worm eggs from Japan, and are willing to distribute a portion of the same to other societies, also to private individuals, who can satisfy the Society that they have a sufficient quantity of mulberry leaf of their own growing to sustain the worms properly. It is hoped that the attention of colonists will be given to propagating the mulberry.

*The Ovipositor in Spring-tails and their Kindred.*—Mr. L. S. Packard describes, among other structures, one of the above, which is rather novel. He is disposed to consider it an ovipositor. In the genus Achorutes it may be found in the segment just behind the spring-bearing segment, and situated on the median line of the body. It consists of two squarish valves, from between which project a pair of minute tubercles, or blades, with four rounded teeth on the under side. This pair of infinitesimal saws remind one of the blades of the saw-fly, and he is at a loss what their use can be, unless to cut and pierce so as to scoop out a place in which to deposit an egg. It is homologous in situation with the middle pair of blades which compose the ovipositor of higher insects, and if it should prove to be used by the creature in laying its eggs, we should then have with the spring an additional point of resemblance to the Neuroptera and higher insects, and instead of this spring being an important differential character, separating the Thysanura from other insects, it binds them still closer, though still differing greatly in representing only a part of the ovipositor of the higher insects.

*The Tarsi of Dytiscus.*—In a very important paper on this subject, by Mr. B. T. Lowne, M.R.C.S., in the "Monthly Microscopical Journal" for June, there are some interesting statements. It says that there are about 200 disk-bearing hairs on the anterior dilated tarsus of *Dytiscus marginatus, punctulatus,* or *circumflexus,* the three common British species. Two of the disks on each tarsus are of remarkable size, and differ in several points beside size from the remainder. The largest of the large disks is situated on the posterior portion of the proximal tarsal joint, close to the tibia, and usually measures $\frac{1}{16}$th of an inch in diameter. The smaller is about half this diameter, and is situated in front of the larger. The other disks cover the remainder of the dilated portion of the tarsus, and are not more than about $\frac{1}{150}$th of an inch in diameter. The middle pair of tarsi have their under side clothed with a dense pad of disk-bearing hairs similar to the latter. If the integument of the upper surface of one of the anterior dilated tarsi be carefully removed, the cavity of the tarsus will be found to be occupied, in great part, by a large but delicate sac. The main trachea with its sacculus and the tendon of the last tarsal joint will be seen lying upon it, and these occupy all the remaining space in the cavity of the tarsus. The sac will be found full of a gelatinous viscid substance, the same as that which exudes from the hairs. It is well supplied with tracheal vessels from the main tracheal trunk, indicating that it is an active secreting gland. If a portion of the upper part of the sac be removed, and its contents cleared away with a camel's-hair pencil, the internal orifices of the disk-bearing hairs of the pulvillus will be seen distinctly in the centre of nipple-shaped projections of the integument which project into the sac.

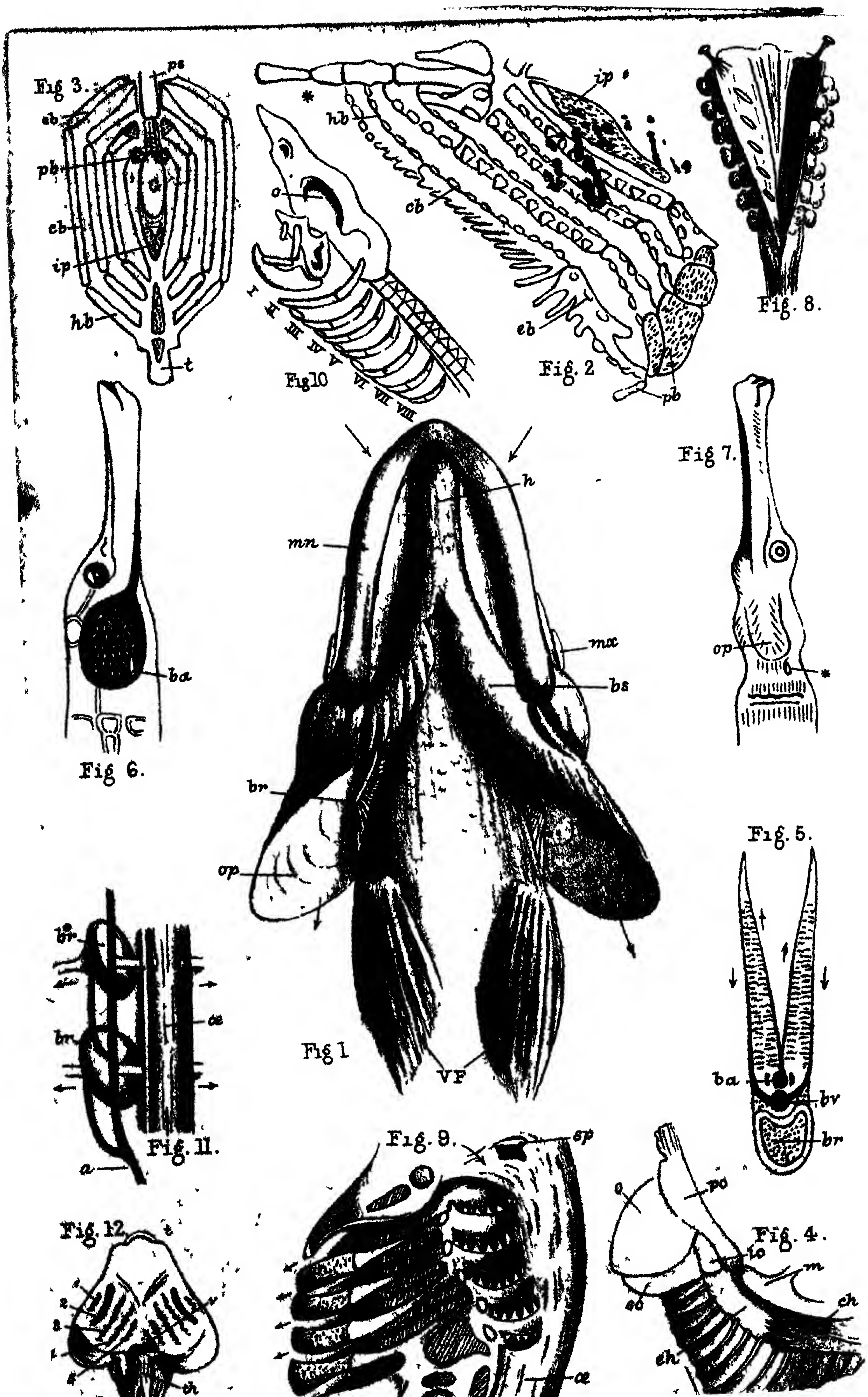
*The Oyster Culture.*—A valuable report on the present efforts in this direction will be found in the "Journal of the Society of Arts," May 5.

*Curious Habits of the Capelin.*—The following note appears in the "American Naturalist" (April). The Capelin (*Mallotus villosus*), an inhabitant of the northern seas of the Atlantic coast of America, is well known as a bait for cod-fish. It visits the shores of America during August and September, for the purpose of spawning, when it is so abundant as to darken the sea for miles. There are some peculiarities about the method of its spawning; the females, on approaching the beach, being attended by two males, who hold the female between them, by means of the ridge of closely set, brush-like scales with which the males alone are provided, so that she is almost entirely concealed. In this state the three run together with great swiftness upon the sand, and in this act the spawn issues from the female, which is simultaneously fertilised. An immense business is carried on in the capture of the capelin as bait for the cod, the French fishermen alone obtaining, from the fishing-ground off Newfoundland, from sixty thousand to seventy thousand hogsheads annually for this purpose.

*Opossum Skins for Gloves.*—Considerable purchases, says the "South Australian Register," have lately been made of opossum skins, for shipment to England, for the manufacture of gloves. The Australians appear to be very glad, for opossums are a frightful nuisance to gardens, crops, &c.

*Risso's Dolphin.*—At the meeting of the Zoological Society, June 6, Professor Flower, F.R.S., gave a description of a specimen of the so-called Risso's dolphin, which had been taken in a mackerel-net near the Eddystone Lighthouse, and of a second specimen of the same dolphin subsequently purchased in Billingsgate Market. After a searching investigation of the history of this supposed species, Professor Flower came to the conclusion that the differences usually held to separate it from the *Delphinus griseus* of Cuvier were untenable, and that the species should be correctly designated *Grampus griseus*.

*Prizes for the Economic Entomology of Great Britain.*—The following prizes for collections of economic entomology are offered by the Royal Horticultural Society:—1. A prize of 10*l.* for the best collection of British insects injurious to any one plant, as the oak, pine, cabbage, wheat, &c. (the choice of plant to be left to the competitor). The insects to be shown as much as possible in their various stages of development—eggs, larva, chrysalis, and perfect insect. In judging, a preference will be given to those collections which most successfully illustrate the life-history of the insect, and exhibit the mischief done, whether shown by specimens, drawings, models, or other means. Examples of the application of drawings, models, and specimens to this purpose may be seen in the Society's collection in the South Kensington Museum. 2. A second prize of 3*l.* for the second-best collection. 3. A prize of 5*l.* for the best miscellaneous collection of any branch of British economic entomology, similarly illustrated. 4. A second prize of 2*l.* for the second-best collection. The collections to be sent to Mr. James Richards, assistant-secretary, Royal Horticultural Society, on or before May 1, 1872, each collection bearing a motto, and a separate sealed envelope, with the motto on the outside, and the name of the competitor inside. The Society is to be entitled to take from any of the collections sent in, whether successful or not, whatever specimens or illustrations they may choose, at a price to be fixed by the judges. The judges to have power to refrain from awarding the prizes should the collections seem not worthy.



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# HOW FISHES BREATHE.

By JOHN C. GALTON, M.A., M.R.C.S., F.L.S.

Late Lecturer on Comparative Anatomy at Charing Cross Hospital.

[PLATE LXXVI.]

"Solemne est Naturæ, unica actione, et eodem instrumento plura commoda acquirere. Hoc præcipue in respiratione observatur."—Borelli.

THE common expression, "to drink like a fish," contains much latent truth, though this animal seems hardly, at first sight, to furnish an apt illustration of excess, seeing that, living as it does in water, it must perforce practise temperance, if by temperance abstinence from all fermented fluids be understood.

Nevertheless, a fish "drinks hard"—as often, say, as twenty times in a minute*—but not in the sense that we understand by drinking, as a compensation for loss of water from the body by various means, and through various channels, or merely to satisfy, toper-like, the capricious cravings of a "thirsty soul." No; most of the water which enters so constantly into its mouth never reaches the stomach, but, after rippling over those beautiful organs, its gills, passes straightway away from the body, having, in this short period, by purifying the blood, renewed the lease of a life which is held by a tenure only too slender—a result the converse of which more commonly attends the more potent draughts, often as periodic, but luckily less frequent, of certain "higher animals."

As the subject of the present article is rather the mechanical than the minutely anatomical, physiological, and chemical processes involved in the breathing of fishes, a very brief summary of the circulation of their blood must suffice.

The blood, which has become befouled and impoverished in its journey through the various organs of the body, finds its way to the heart—the great pumping-station of the physiological sewage in the body of a fish—by means of two longitudinal trunks, one on either side, which meet, each through the medium of a transverse channel, in a large antechamber,

* A pike which the author timed, the other day, at the Gardens of the Zoological Society was gulping in water at about this rate.

which also receives, through a special canal, the blood from the digestive organs. This antechamber communicates directly with the two-chambered heart, which corresponds—in function, at any rate—with the right half of the four-chambered heart of warm-blooded animals. The blood, propelled by the contractions of the muscular walls of the second chamber, or ventricle, of the heart, and of the root ("conus arteriosus," Fig 6, *b a*) of the main trunk proceeding from the heart, next finds its way, by means of the branches of this main channel, into the leaves of that very compendious volume on respiration—the gills ("branchia"), and then discharges its ballast, carbonic acid, for a better cargo, oxygen.*

Vein-roots, as many in number as the arterial branches which conveyed the impure blood to the gills, collect and carry with them the regenerated fluid to a kind of rendezvous at the base of the skull—the "aortic circle" ("circulus cephalicus")—from which the greater part flows on in a single main trunk, or aorta, to be distributed, through various branches, to various parts of the body.

Having thus very briefly considered the means by which the blood is brought to the gills, there to exchange the old for that which is new, the impure for the pure, and the deadly for that which sustains life, we will next proceed to study more in detail the process by which water is admitted to the gills, and the way in which these organs first make the most, and afterwards rid themselves, of the streams which bathe their fringing folds.

If a fish—for instance, a pike—be observed under fairly natural conditions, e.g. in the water of a large aquarium, it will be seen from time to time to make a kind of gulping, swallowing movement with its mouth. After the jaws have been opened, there is a slight, but appreciable, pause, after which a whitish fleshy fold or curtain† will be seen to descend from the interior, not the edge, of the upper jaw, a little

* It must be borne in mind that the gill-laboratory of the fish does not rob, as by a sort of electrolysis, the water of that oxygen which is one of its two elementary constituents, but only of the *free* oxygen derived from the air held in solution by the water. Although the quantity of free oxygen present in water is much less, volume for volume, than that contained in air, seeing that the source of such oxygen is the air itself, held in solution in the water, this imprisoned air is, on the other hand, somewhat richer in oxygen than ordinary atmospheric air, for the reason that oxygen is twice as soluble as nitrogen in water.

† Cuvier's description of this structure is so good that it is worth while to transcribe it here in full:—"Il y a généralement en dedans de chaque mâchoire, derrière les dents antérieures, une espèce de voile membraneux ou de valvule formée par un repli de la peau intérieure et dirigée en arrière,

behind its tip, and to be met simultaneously by a similar screen which rises from behind the tip of the lower jaw. This, as Cuvier suggests, has probably the effect of preventing any reflux of the water which has just been taken in, and possibly, which Professor Owen doubts, the escape also of food from the mouth.

Simultaneously with the appearance of this curtain, or "velum," a peculiar kind of spiny ruff (*b s*, Figs. 1 and 4), presently to be described, seen around the throat, so to speak, of the fish, expands, and almost immediately the mouth is shut.

The above action, seemingly so simple both to witness and to describe, presents one difficulty and complexity to the observer, viz. that he has to keep his eye on two points which are somewhat wide apart, namely, the "velum" in the mouth and the prickly ruff around the throat of the fish. Hence may result an error differing only from the "personal equation" of the astronomer, in that it concerns but one sense, for which some allowance must be made.

As the observer, unless he has no respect unto his fingers, will not care to make very prolonged explorations into the interior of the mouth of a living fish, at all events of the kind which has been instanced, he must content himself with making further observations on a dead specimen.

In considering the various modifications of the breathing organs of fishes, it will be found practically convenient to follow the arrangement adopted by Prof. Owen in his Hunterian Catalogue of the contents of the Museum of the Royal College of Surgeons.

*a. The respiratory currents enter by the mouth and are expelled by an orifice on each side.** If we take a pike and look down its widely opened mouth, we shall see at the back of the throat something like that which has been represented, diagram fashion, in Fig. 3. At the front of the floor of the mouth will be seen a small tongue, not flexible and protrusive as in ourselves, but having its range of motion limited by that of the median chain of bones of which it forms the tip. On either side of the narrow floor, which is carpeted here and there (see dotted part of the figure) by rasp-like sets of very

dont l'effet doit être d'empêcher les alimens et surtout l'eau avalée pour la respiration, de ressortir par la bouche."—*Histoire Naturelle des Poissons*, vol. i. p. 497.

This organ, from a kind of remote analogy to the *velum pendulum palati*, a structure which, with the tonsils, is absent in all fishes, may conveniently be termed "velum."

* Fishes in which this occurs were once termed, for reasons which we shall see presently, "pisces branchiis liberis." Most of them have a bony skeleton.

fine teeth, rise four jointed columns, the last pair of which form the door-posts, so to speak, of the gullet. The spaces which are necessarily left between these pillars are not filled up by any membranous wall, but serve severally as inlets to a chamber on either side, which, in that it lodges the gills, may conveniently be called "branchial." Each branchial chamber has further its communication with the world of waters without carried on or cut off by the opening or shutting of a door or lid—the "operculum" (*o p*, Fig. 1; *o*, *s o*, *i o*, *p o*, Fig. 4)*—hinged on behind the eye and put in action by a special set of muscles.

To return for a short time to the pillars just mentioned. Each of these is hinged on below—the word will be better appreciated presently—to certain of the median chain of bones (see Fig. 2) which carry the tongue at their tip; and further consists of a variable number of pieces—at most four—also hinged on to one another, which have been termed (proceeding from below upwards) *hyo- cerato- epi-* and *pharyngo-branchials* (*h b*, *c b*, *e b*, *p b*, Fig. 2). The two first pair of pillars possess all these members, while the third and fourth have a hyo-branchial in common; the pharyngo-branchials of the first pair are, further, small and slender bones, while those of the succeeding three pairs are broadened out, and are armed on their lower surfaces, i.e. those which look downward into the pharynx, with fine teeth.

Since each pair of pillars, through being made up of jointed pieces, makes a very appreciable but varying curve with the floor and roof of the throat, they have been not inaptly termed "arches."† The three lower members of each arch further carry on their concave (inward-looking) aspect, two rows of tooth-like projections (see Fig. 2), which vary much in shape and arrangement in the different species of fish; while on their convex (outward-looking) surface they are grooved for the lodgment of the gill-plates (Fig. 5), which are arranged, comb-like, in a double row on the first three arches, but, in many cases, in single series only on the fourth. The foundations, lastly, of a rudimentary fifth arch are represented on either side by the so-called "inferior pharyngeal bones" (*i p*, Figs. 2 and 3).

Before leaving the arches we must notice certain important

* Lat. *operculum*, a cover, lid; from *operio*, I cover. The same word is applied to the door by which the whelk, periwinkle, and other snail-like molluscs close the entrance to their shells.

† For the latest views on the homologies of these branchial arches—a most interesting subject, but one which does not come within the scope of this article—the reader is referred to a paper by Mr. St. George Mivart, F.R.S., "On the Vertebrate Skeleton," published in the last volume of the *Transactions of the Linnean Society*.

purposes which are served by their jointed arrangement. 1.* Each half-arch, in being set on at an angle with the median chain of tongue-bearing bones (see Fig. 2), can, by the action of special sets of *levator* and *depressor* muscles,* be made to form a greater angle with this middle base line, and so simultaneously bring about both the widening of each entire arch in its transverse diameter, and the increase of the space between it and its neighbours in front and behind. 2. It is possible for the upper and lower pharyngeal teeth-plates (*p b*, *i p*, Figs. 2 and 3) to act upon each other, and so be the means of a mastication of food in some sort.†

Let us now briefly turn our attention to the spiny ruff which surrounds the throat of the fish. The bulk of this is formed on either side by two bones, the *epi-* and *cerato-hyals*, jointed on, one behind the other (*e h*, *c h*, Fig. 4), the former and most posterior of which is brought into relation with a bone—the "hyo-mandibular"—which is the chief support of the lower jaw, through the medium of a small bone, the "stylohyal;" while the most anterior, the "cerato-hyal," is hinged on to the chain of tongue-bearing bones, not far behind its tip (* Fig. 2). This arch, like those which carry the gills, has, therefore, a very fair range of motion. The epi- and cerato-hyals further give support to certain curved bones (*b s*, Figs. 1 and 4), which afford attachment to an intervening membrane, in the manner of the ribs of an umbrella. These vary in number‡ and in mode of attachment, being hung on, sometimes to the outer, sometimes to the inner sides of their supports—why, it is not easy to say—and are called, from their office, "branchiostegal rays."§ From ray to ray of this apparatus passes a muscle, which is attached behind to the inner surface of the operculum. This, by raising the rays, and acting at the same time in antagonism to a depressor muscle which rises from the cerato-hyal of the opposite side, and is inserted into the foremost of the rays, spreads out the branchiostegal membrane umbrella-fashion. "These muscles," as

* The levators, depressors, and retractors of the branchia. See Cuvier, *Hist. Nat. des Poissons*, tome i. pl. v. and vi.

† "The necessary co-operation of the jaws," observes Prof. Owen, "with the hyoid arch in the rhythmical movements of respiration is incompatible with protracted maxillary mastication; and, accordingly, the branchial apparatus renders a compensatory return by giving up, as it were, the last pair of its arches to the completion of the work which the proper or anterior jaws were compelled by their services to respiration to leave unfinished."

‡ In the *Syngnathus* (Figs 6 and 7) they are entirely wanting. *Polypterus* has but one, and there are only three present in *Cyprinus*.

§ From the Greek βράγχια, gills, and στέγειν, to cover.

Professor Owen observes, "regulate the capacity of the branchial chamber, and mainly act upon the water it contains."

The only point to be noted about the operculum is, that it consists of four elements (see Fig. 4) fastened together, so as functionally to be one scale-like bone. This can be set ajar, door-wise, or be brought close to the head by the action of special muscles; the hinge, so to speak, being at its anterior edge.

Having now described the apparatus for branchial breathing, let us take an anatomical glance at the method by which a single respiratory act is effected in an osseous fish.

This act "differs," as Professor Owen well observes, "from that of swallowing, only in the streams of water being prevented from entering the gullet, and being diverted to the branchial slits on each side the pharynx." First, the mouth is opened by the drawing back of the maxillary bones, and the pulling downwards of the mandible by their proper muscles, while at the same time the two "rami," or branches, of the latter bone are separated wider behind by the action of certain muscles on its supports (the "hyo-mandibular" arch). The result of this latter act is the widening of the branchial cavity, the space of which is further increased by the opening of the opercular doors, and by the spreading out of the branchiostegal rays, and consequent stretching of the membrane extended between them. The branchial arches are, moreover, drawn forward by the action of their levator muscles, i.e. are forced to make a greater angle with the median chain of bones than they do when at rest. During this time the water, whose reflux from the mouth is barred by the action of the "velum" already described, rushes through the teeth-guarded sluices* of the branchial arches, and washes over the gill-plates, freely distributing the stores of life with which it is laden. Next, the fissures leading to the gill-chamber are closed by the depression, through the action of special muscles, of the branchial arches, and by the pushing forward of the cerato-hyals by muscles which pass from these bones to the inner side of either branch of the mandible, not far from its tip.† The water, then—being pent up in the gill-chamber, and all return to the

* Were it not for these teeth the fish would be speedily choked by various substances going "the wrong way" into the gill-chamber. The Mullet, which takes in a quantity of sand and mud with its food, and works it about between its pharyngeal bones, has a number of extremely delicate fringes attached to the concavity of its branchial arches.

† The reciprocal action of these muscles—the homologues, according to Cuvier, of the *geniohyoidei* in ourselves—is extremely interesting. When the cerato-hyals are the fixed points from which they act, they depress the mandible; when, however, this latter bone becomes the fulcrum, they pull forward the hyoid apparatus, through the medium of the cerato-hyals.

mouth being cut off—is forcibly driven out backwards, again to join the waters of river or sea, by the speedy folding up of the branchiostegal membrane, and by the shutting of the opercular doors upon sills formed behind them by the bones which carry the pectoral fins.

In the operation of "drowning" a hooked fish, which is familiar to every angler, the opercula of the fish which is being towed down stream must be either pressed forcibly against the sides of the head or be forced widely aside, the result of which is to embarrass, and, ultimately, put an end to, respiration; in the first case, by the hindrance to the proper distension of the branchial chambers, and the consequent absence of a vacuum—if such an expression can be used—which shall be filled by the water taken in at the mouth, not to mention the want of room for the play of the gills; in the second instance, by the retention of the water, now useless for breathing purposes, in the gill-chambers, since the doors which ought to force and shut it out can no longer do their work.

The size of the outlets of the branchial chambers seems to bear an inverse ratio to the length of time during which a fish is able to breathe when taken out of the water; for in those fishes, such as the mackerel and herring, which die very soon after removal from the water, these openings are relatively large; while in the eel tribe, which not only can exist for some time after being landed *nolens volens*, but sometimes make "on their own hook" considerable overland excursions, the outlet from the gill-chamber "is a small vertical fissure, situated at some distance behind the gills; the branchial cavity is therefore proportionately elongated, and the escape of fluid from it is consequently impeded."

The Anabas (*Perca scandens*), which, on the authority of a Danish lieutenant,* is yet believed by many to make somewhat purposeless expeditions up the trunks of palm-trees, can, undoubtedly, whether it climb or not, exist for a long time out of water. Dr. Hamilton writes thus of this fish: "Of all that I know, the cobojius is the fish most tenacious of life in the air; and I have known boatmen to keep them for five or six days in an earthen pot without water, and daily to use what they wanted, finding the fish as lively and fresh as when caught. In fact the Calcutta market is chiefly supplied from extensive marshes in the Yasor district, and about 150 miles distant. From thence boat-loads are brought, kept alive without water until sold."†

This fish, together with certain others, e.g. the "Gourami" (*Osphromenus olfax*) of the Isle-de-France, has, as Cuvier has

* Daldorff. (See *Trans. Linn. Soc.*, vol. iii. p. 62.)

† *Fishes of the Ganges*, vol. i. p. 99.

well represented,* its epi- and pharyngo-branchials enormously developed and thrown into complex folds, covered with membrane, which serve to retain a sufficient amount of moisture.

Sir J. Emerson Tennent describes a remarkable habit which some of the Ceylon fishes have of burying themselves in the earth, in the dry season, at the bottom of the exhausted ponds, there to await the renewal of the water at the change of the monsoon—an expedient to which the crocodile also is said to resort.† He also states that in those parts of Ceylon where the country is flat and small tanks are numerous, the natives are accustomed in the hot season to dig in the mud for fish.

The Cuchia (*Amphipnous*) and the Singio (*Saccobranchus*) both fishes of eastern climes, and both extremely tenacious of life, also present curious modifications of the branchial apparatus, which cannot here be noticed in detail.

Having considered somewhat at length, on Aristotle's principle of proceeding ἀπὸ τῶν γνωριμῶν, the first and more general method of water-supply to the gills, not much space is left for description of the two remaining processes.

*β. The respiratory currents enter the mouth and are expelled by five orifices on each side.*‡

The great bulk of the so-called cartilaginous fishes, namely, the sharks and rays,§ afford an instance of this mode of breathing.

The branchial arches in these fishes, more or less cartilaginous in substance, are not hung, so to speak, from below the base of the skull, as are those (Fig. 3) of bony fishes, but are attached (see Fig. 10) to the sides of the front vertebræ of the trunk. No branchiostegal or opercular apparatus is functionally present; but from the branchial arches (seen in section in Fig. 9) proceed long partitions, or septa, on either side of which are attached the gill-plates,‖ and which form the walls of chambers, or sacs,

* *Hist. Nat. de Poissons*, tome vii. pl. 205. The land-crabs, which are actually *drowned* if kept in water, have their gills moistened by the watery contents or secretion of a spongy organ situated in the gill-chamber.

"The gills of fishes," as Professor Marshall well observes, "are not ciliated on their surface; but it is necessary that they should continue moist, or the usual respiratory interchanges between the blood and the air dissolved in the water would soon cease. Respiration will, however, go on for a short time in the air, provided that the gills remain moist."

† *Ceylon*, vol. i. p. 218. In the same volume is given a very interesting account of the writings of various ancient authors, e.g. Aristotle and Theophrastus, on the subject of the Migration of Fishes over Land.

‡ The fishes in which this form of respiration occurs were formerly termed "pisces branchiis fixis."

§ Division *Plagiostomi* of the order *Elasmobranchii* (ἔλασμα, a thin plate, and βράγχια, gills).

‖ For the *rationale* of the arrangement of the gills see the explanation of Fig. 11.

each of which communicates by fissures both with the gullet and with the water without. With two exceptions, the names of which—*Hexanchus* and *Heptanchus*—are sufficiently explanatory, there are five gill-sacs present, the hinder of which contains only a single gill, attached (see Fig. 9) to its front wall.

The water taken in for respiratory purposes is, in default of a branchiostegal and opercular machinery, directed and made to move on by muscles and elastic structures which act more or less directly upon the gill-chambers and their fissures of entrance and exit.

Is it not possible—this is merely thrown out as a suggestion—that this peculiar mechanism for breathing, so different in action from, though but a modification in structure of, that in bony fishes, may, apart from all considerations of embryology or of correlation of growth, bear some relation to that peculiar habit the fishes in which it occurs have of turning on their backs when they take their prey—a proceeding necessitated by the position of the mouth on the ventral surface of the body?

Sharks, in their fœtal state, have temporary external gills, consisting of numerous elongated filaments, which project from the branchial apertures, immediately in front of the pectoral fins.*

*γ. The respiratory currents pass both in and out of the external branchial apertures.*

The breathing apparatus of the Lampreys and certain marine fishes whose very name is sufficiently repulsive, the Glutinous Hags,† comes under this category. In these fishes the gills consist of closed sacs (*b r*, *b r*, Fig. 11)—not answering, however (see explanation of Fig. 11), to the gill-chambers of sharks and rays—thrown into folds in their interior. These, in the Hag, have tubular connections on either side with a duct which opens upon the belly of the fish. Betwixt the openings of the two ducts lies a third pore, which communicates with the gullet, which, in its turn, has a separate tubular communication (see Fig. 11) with each gill-sac. The water, then, through the middle orifice passes into the gullet, traverses the gill-chambers, and finds its way out by the two ducts. The sacs, however, have each a small duct which opens by a distinct orifice in the skin in the species of Hag called from this circumstance *Heptatrema* (see Fig. 11), while in the lampreys, besides possessing

* See Preparation No. 1061, Royal College of Surgeons.

† The Hags, like a certain section of the human community which lives upon those of its fellow-mortals who are "in difficulties," bore into the bodies of other fish, entering the mouths of their victims when they are struggling on the hook of the fisherman.

The Lampreys and Hags, once called, from the formation of their mouths, *Cyclostoms*, are now included under the term *Marsipobranchii* (μάρσιπος, a pouch, and βράγχια, gills).

a similar set of seven "stigmata" on either side, they communicate internally with a median canal, which ends blindly behind, but runs forward beneath, and totally distinct from the gullet, to communicate with the pharynx through the medium of a valve-guarded opening.

The above curious modifications of the breathing apparatus have an evident relation to the pre-occupied state, so to speak, of the mouth in the fishes under consideration; that of the Hag being commonly buried in the flesh of its "host," while that of the Lamprey, if not similarly engaged with prey, serves, suckerwise, to anchor its waving owner to some stone or other support.*

A delicate cartilaginous trellis-work—the parts of which, according to Mr. Mivart, are not homologous with the branchial arches of bony fishes—surrounds the branchial apertures in the Lamprey, and, moreover, in some degree encases and protects the heart.†

The leading varieties of all bonâ-fide fishes having been passed in review, only two somewhat aberrant forms, which occupy positions on the boundary line of the class, or, one is almost tempted to say, the topmost and lowest rungs respectively of the fish-ladder, claim, in conclusion, some slight notice. To begin with the lowest form, viz., the Lancelet (*Amphioxus*).

This little fish, which seems to be the very roughest sketch of a vertebrate animal, with a brainless nerve-system, blood-vessels which lack a heart, and a backbone devoid of vertebræ, respires in this wise: water is taken in at a mouth surrounded with fringes, and, after passing, gently urged along by cilia, through windows—between the panes of which are pulsatile vessels—perforated in the pharynx, into a cavity answering to a branchial cavity, finds its exit by an orifice pierced through the ventral surface of the fish.

The mechanism of respiration here seems almost identical with that in animals a little lower than the Mollusca, namely, the Ascidia, or "sea-squirts" (see *Popular Science Review*, July, 1869), creatures which, according to recent researches, seem, in early stages of development, to make somewhat startling approaches to animals once thought to be separated from them by an impassable gulf and by well-defined barriers.

* It is stated in one of the Hunterian catalogues (*Physiology*, vol. ii. 1029) in the Royal College of Surgeons, that if a Lamprey, when sticking to the side of a vessel, "be held with one series of apertures out of the water, the respiratory currents are seen to enter by the submerged orifices, and, after traversing the corresponding sacs and the pharynx, to pass through the opposite branchia and to be forcibly ejected therefrom by the exposed orifices."

† Preparation 34, Royal College of Surgeons.

The higher of the two aberrant forms, namely, the Mud-fishes of Africa and South America, which once occupied a yet more exalted place among the Amphibia, have the choice of two methods of respiration, in that they possess a pair of rudimentary lungs, in addition to their branchial apparatus.* These functional lungs, which are, undoubtedly, a modified bilobed air-bladder, have a cellular structure, "the cells having the same proportional size and form as in the respiratory part of the lung of a serpent," receive through a pulmonary artery, derived from two of the aortic arches which have no gill-structure developed upon them, impure blood, and return it purified through pulmonary veins, and communicate with the gullet by a glottis-like aperture (see Fig. 12), guarded by a cartilage.

"The peculiar modification of the gills," observes Professor Owen, "and air-bladder of the Lepidosiren, are precisely those which adapt them to the peculiar conditions of their existence.† In the inactive state into which they are thrown by their false position as terrestrial animals, the circulation, which would have been liable to be stopped had all the branchial arteries developed gills, as in normal fishes, is carried on through the two persistent primitive vascular channels. When the lepidosiren resumes its true position as a fish, the branchial circulation is vigorously resumed, a large proportion of arterialised blood enters the aorta, and both the nervous and muscular systems receive the additional stimulus and support requisite for the maintenance of their energetic action."

Does not such a form, together with those Amphibia which, though possessing perfect lungs, yet retain gills, seem in some measure to bridge over the gulf between the Fish and the higher Vertebrata, the highest even of which retains in ripe years a souvenir, so to speak, of a condition structurally more fish-like, in the shape of a persistent—gill-less, it is true—branchial cleft?‡

*. For this reason the mud fishes are classed together under the term *Dipnoi*.

† This fish is accustomed, during the dry seasons, to bury itself in the indurated mud of the river-banks, and to remain in a state of torpor until the return of the rains.

‡ Pseudo-branchia, opercular and spiracular gills have not been noticed in detail, since, though interesting from a homological point of view, they do not play an important part in the respiratory function.

For fuller details of the varieties of the air-bladder in fishes the reader is referred to an article by the Rev. W. Houghton in the *Popular Science Review* for October, 1868, and to Fischer's *Versuch über die Schwimmblase der Fische* (Leipzig, 1795).

Mr. Herbert Spencer, in the second volume (pp. 321 *et seq.*) of his *Princi-*

## DESCRIPTION OF PLATE LXXVI.

FIG. 1. View of under surface of the head and of the fore part of the body of a Trout (*Salmo Fario*), from a sketch taken by the author from a freshly-caught specimen. All the parts have been left in their natural relations to one another, save that the gill-covers (*o p*) have been drawn aside in order better to display the branchiostegal rays (*b s*) and the first pair of gills (*b r*).

*m n* Mandible, or lower jaw.

*m x* This is not the eye, as might at first be supposed, but the lower part of the maxilla, or upper jaw.

*h* The median portion of the hyoid arch, upon which the carriers of the branchiostegal rays abut on either side.

V F. Ventral fins.

FIG. 2. Branchial arches of Perch (*Perca fluviatilis*), seen from above; after Cuvier (*Histoire Naturelle des Poissons*, tome i. pl. iii. Fig. 7).

Only the left halves of the arches, with the median elements ("Copulæ," "Verbindungstücke") of the hyoid apparatus are represented. Each half arch is seen to be made up of four factors, which are, from below upwards, as follows:—

*n b* Hyobranchial.
*c b* Ceratobranchial.
*e b* Epibranchial.
*p b* Pharyngobranchial.

The three first, with the exception of the hyobranchial element of the rudimentary fifth arch, all carry gill-plates on their convex edges, while the edges which look inwards towards the gullet bear two rows of tooth-like projections.

The pharyngeal factor on either side of the first arch is slender and style-shaped, while those of the three succeeding arches are broadened out, and carry on their lower surfaces very fine teeth ("dents en velours"). A similar armature may be seen on the upper surface of the only representative of the fifth arch—the inferior pharyngeal bone (*i p*) of either side.

In this figure, which is somewhat diagrammatic, the two upper elements, namely, the epi- and pharyngo-branchials of each arch, have been spread out in the same plane with the lower factors of their respective arches. Naturally, they make with the said factors a kind of curved right or obtuse angle, a relation which is better seen in the next figure.

The point at which the "cerato-hyal" (see Fig. 4) of this side joins the median chain of bones is indicated by the sign *.

FIG. 3 is taken from a Hunterian specimen (No. 50) in the Museum of the

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*ples of Biology*, puts forth a most ingenious theory of the origin of this structure; any dissentient from which can turn to the second chapter of Mr. St. George Mivart's *Genesis of Species*, which deals with "the incompetency of 'Natural Selection' to account for the incipient stages of useful structures."

Royal College of Surgeons. It is a semi-diagrammatic rendering of the branchial arches and their relations to the roof and floor of the mouth in the Pike (*Esox Lucius*) as they are seen through the widely-opened jaws of the fish.

In the endeavour to make plain as much as possible which lies in a confined space, some violence has been done to perspective. Nevertheless, the drawing is fundamentally correct. In order that the figure may not be more obscure than it is feared it may possibly be already, the gill-plates and inwardly-directed fringes of teeth have been omitted.

*G* Tube of the gullet.

*t* Rudimentary tongue.

*p s* Parasphenoid bone.

The rest of the letters have the same signification as those in the preceding figure.

The dotted parts of the diagram indicate those regions of the threshold of the gullet which carry a rasp-like armature of small teeth.

FIG. 4. Taken from part of the right side of the specimen—a dried and varnished head of a Pike—on which the preceding diagram is based. The operculum and branchiostegal rays and membrane are here fairly shown.

*o* Operculum.

*s o* Suboperculum.

*p o* Preoperculum.

*i o* Interoperculum.

*c h* Ceratohyal, which joins anteriorly the median, tongue-bearing basihyal.

*e h* Epihyal, which is joined on behind to the slender stylohyal, which, in its turn, articulates with the hyo-mandibular bone: all which relations are concealed by the overlapping operculum.

The cerato- and epi-hyals are each seen to carry seven branchiostegal rays (*b s*), the former on its inner, the latter on its outer side.

*m* Lower part of mandible. A flexible membrane, better seen in Fig. 1, fills up the gap between it and the upper edge of the cerato- and epi-hyals.

FIG. 5. After Cuvier (*Hist. Nat. des Poissons*, tome i. pl. viii. fig. 6), represents a transverse section of the gill-bearing elements of a branchial arch of a Perch, together with its attached gill-plate ("feuillet," Cuv., or "branchial lamina"), which is seen to be made up of two leaflets.

*b r* Bone of the arch in section.

*b a* Branchial artery in section. It may be observed to divide into two branches, one for each gill-plate, each of which courses along the inner edge of a leaflet, giving off in its course numerous slender twigs. The purified blood is returned to the branchial vein—which is seen in section at *b v*—by two trunks which run each along the outer edges of the two leaflets which are the components of a gill-plate, having been first collected in small vessels which meet the arterial twigs on the mutual terri-

tory of the capillary system. The carrier of the purer blood is seen to occupy a deeper and, presumably, a safer position than does its fellow, at the bottom of the groove which is channelled out along the convex edge of its bony support.

The arrows indicate the course of the blood.

FIG. 6. Head of a species of Pipe-fish (*Syngnathus Æquoreus*) from a preparation (No. 1041) in the Museum of the Royal College of Surgeons.

The operculum on the right side has been removed, in order to display the gills, which are "composed of a double series of tufts on each of the four arches."

*b a* Bulbus arteriosus—the root, so to speak, of the trunk which gives off an arterial branch for the gills of either side.

FIG. 7. Head of another specimen of the Pipe-fish (*Syngnathus Sp : ?*) captured by the author in an oyster-dredge off Seaview, Isle of Wight.

*o p* Operculum, left side. Above and behind it is seen the small outlet (*), just large enough to admit a bristle, for the water which has bathed the gill-tufts.

FIG. 8. The pharynx of the same specimen opened out in order to show the five fissures on either side which admit water to the tufted gills.

FIG. 9. Much reduced from a sketch of a preparation (No. 1060) in the Museum of the Royal College of Surgeons, of the head of the Gray Shark (*Galeus communis*). The right half of the head, through which a vertical longitudinal section has been made, is preserved in the preparation. In the pharynx five openings are seen, which, guarded by tooth-like projections, not bony as in osseous fishes, but more or less cartilaginous, allow of the influx of water, which, after bathing the gills paired in their chambers, finds exit by five fissures at the side of the head.

*œ* œsophagus, or gullet.

*s p* opening leading to the canal which terminates externally at the so-called "spiracle" (Spritzloch").*

The arrows indicate the direction of the water swallowed for respiration.

FIG. 10 is a diagram (Schema) after Gegenbaur (*Grundzüge der Vergleichenden Anatomie*, 2nd edit. fig. 210) of the cartilaginous skull and "visceral" skeleton of one of the *Sclachii*, e.g. Sharks and Rays.

*o* orbit, for lodgment of the eye.

I. II. First and second visceral arches, the former of which forms the boundary of the mouth, while the latter ("hyoid") carries the tongue.

III.—VIII. Branchial arches, comprising the rest of the visceral skeleton.

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* The remnant of the first "visceral cleft," which in the bony fishes becomes closed up, but in ourselves is partly persistent as the external canal of the ear, and the Eustachian tube.

In *Heptanchus* there are nine or ten visceral arches.

FIG. 11. Taken from a preparation (No. 1018) in the Museum of the Royal College of Surgeons, represents two of the six gill-sacs on one side of the Myxine or "Glutinous Hag" (*Heptatrema cirratum*).

These sacs,* *b r b r*, the upper of which is represented in profile, are seen internally to communicate with the œsophagus (*œ*), each by a short tube. Externally, they are brought into relation, by similar means, with one of the two longitudinal canals (not shown in the figure) which discharge the de-oxygenated water by two pores opening on the ventral surface of the body. The lowest arrows show the direction of the water used for respiration, which is admitted not at the mouth, as in other fishes, but at a small opening placed betwixt the two pores which allow of its exit. Each sac is further seen to be supplied with a branch from the branchial artery (*a*) of its side.

FIG. 12 is a view, from above, of the respiratory apparatus of the African Mud-fish, *Lepidosiren* (*Protopterus*) *Annectens*, reduced from a figure in Professor Owen's paper on the anatomy of this fish. (See *Trans. Linn. Soc.* vol. xviii. tab. 26, fig. 1).

The pharynx has been slit up in the middle line, and its sides spread out on either side.

The five branchial fissures are indicated by numbers.

*t* Tongue.

*g* Aperture, or glottis, pierced in the rudimentary thyroid cartilage (*t h*) leading to the short "ductus pneumaticus," or trachea (*d p*), which, in turn, communicates with two lung-like sacs.

*v* is a valve which, though it is not stated to do so by Professor Owen, may act, to judge from the figure, as an epiglottis, i.e. prevent food, &c., from falling into the glottis.

[*All figures founded by the author on specimens in the Museum of the Royal College of Surgeons were drawn by the kind permission of Professor Flower, F.R.S.*]

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* "The leading condition," says Professor Owen, "of the gills in other fishes may be understood by supposing each compressed sac of a Myxine to be split through its plane, and each half to be glued by its outer smooth side to an intermediate septum, which would then support the opposite halves of two distinct sacs, and expose their vascular mucous surface to view. If the septum be attached by its entire margin, the condition of the plagiostomous gill is effected. If the septum be liberated at the outer part of its circumference, and the vascular surfaces are produced into pectinated lamelligerous processes, tufts, or filaments, proceeding from the free arch, the gill of an ordinary osseous or teleostomous fish is formed. Such a gill is the homologue, not of a single gill-sac, but of the contiguous halves of two distinct gill-sacs, in the Myxines. Already, in the Lampreys, the first stage of this bi-partition may be seen, and the next stage in the Sharks and Rays."

This throws much light upon the somewhat puzzling variations of the branchial arteries in fishes. See also Gegenbaur, *op. cit.* p. 809; and the Introduction of Professor Rolleston's *Forms of Animal Life*.

# MR. CROOKES' NEW PSYCHIC FORCE.

By J. P. EARWAKER, Merton College, Oxford.

"SEEING is believing" is an old and much-quoted adage which is by many people so firmly believed in, that with them it quite partakes of the nature of a truism. To suggest for one moment the possibility of their being either partially or entirely mistaken, is to call in question their veracity, their powers of observation, and, in fact, their whole moral integrity, and is usually met, not by convincing arguments, but by indignant reiteration. To question the accuracy of their statements is considered in the light of a personal insult, and for anyone to cross-examine their narrations, however strange these may appear, is to them unbearable. But just as it is the firmest of all legal maxims, that no story can by any possibility be true which cannot stand the test of cross-examination, so it should be the test of all narrations, especially those of an extraordinary character, that they in their turn should stand the most rigid and searching cross-examination to which they can possibly be subjected.

Few ordinary readers of newspapers can have failed to notice how the first published accounts of any special or marked crime are so plausible, and apparently so convincing, that each reader feels there can be no doubt at all as to the guilt of the person accused. They have no means of knowing how far the plausible statements they read bring certain facts into undue prominence, and cast others equally important, but antagonistic, into the shade; but notwithstanding all this, they constitute themselves arbitrary judges on the first evidence that is thrust into their hands. When, however, the case is legally tried, and by cross-examination the evidence is thoroughly sifted, how different the case looks, what an innocent person the unfortunate accused is, what miseries he must have suffered, what imbeciles the police are, and so on, *ad infinitum*. The human mind is the same all the world over, at all times and in all ages—grossly credulous, most easily convinced,

quick to imagine, and equally quick to decide; and yet, when all its boasted judgment and foresight are proved utterly wrong, it is by no means taught by experience. At the next opportunity men commit the same errors of judgment, and again Experience would teach them; but all to no purpose; they cannot and will not be taught by her.

The history of popular delusions is one of the most instructive, and at the same time the most saddening of all historical narratives. The weakness of the so much lauded and boasted human understanding, the firm belief that men have placed, and apparently ever will place, in the mere statements of their fellow-men, the gross and almost unimaginable credulity they are constantly exhibiting, all these make up a picture as saddening as it is true. See how long and how widely the universal belief in astrology, magic, and witchcraft prevailed, and when these beliefs had to give way at the first dawn of true science, it was only to make place for new ones, until, in recent times, almost every ten years has seen a new imposture arise, has seen it in its full glory, and has witnessed its sudden overthrow. To mention but the titles of many of these would, to most of our readers, be but a string of mere names; but if we confine ourselves to the last fifty years only, what a rush of them there is—animal magnetism, the odic force, electro-biology, mesmerism, table-rapping, table-turning, spiritualism, &c. &c. The more apparently scientific the name, the more it was considered likely to take a hold on the public. But just as medical men find such great difficulty in convincing the great mass of mankind that thousands and thousands of lives are annually lost by a belief in quack medicines and quack doctors, so too now it has become most difficult for scientific men to make the outside world believe that the grossest impositions and the most outrageous impostures are constantly being palmed off upon them. Few people have any true idea how outrageous many of these pseudo-scientific delusions really are, and how they, like parasitic fungi, grow at the expense of those who support them.

The history of spiritualism illustrates this well. As far as we can remember, it first dawned upon the world from the other side of the Atlantic, under the shape of table-rapping and table-turning. An ordinary innocent table was stated to be capable of answering any questions put to it, and also of capering about and raising itself in the air, under the influence of some one or more persons whose innocent hands were placed lightly upon it. To account for the first series of these phenomena, some ingenious person called to his aid the spirits of departed persons; the ever-credulous world jumped at this theory, and its originators and followers, having thus struck a

virgin field, proceeded from bad to worse. "Spirits" became far too general a term; special spirits were asked for, and immediately presented themselves, those of well-known and formerly distinguished persons, as might have been expected, being particularly obliging; then this in return became wearisome, and these renowned spirits took to music as a relaxation, and the *séances* were never complete without most melodious and soul-inspiring "spirit-music." From music the step to poetry is not far, and spiritual poetry (such lovely verse and sentiment!) became quite the rage. Then these spirits, ever so considerate and attentive to poor mundane humanity, endued their hands with a corporeal reality, and let their most ardent believers both see and feel them, and moreover, as photography became a science, they so far condescended as to allow themselves to be photographed, and "spirit-photographs" arose, had their day, and disappeared. And so it went on, till, finally, the latest of all spiritual manifestations is, that these obedient spirits come in crowds at the mere bidding of a great medium, they tumble chairs and tables about, they play on accordions, they tickle people with their corporeal hands, they scribble on pieces of paper, they carry the medium through the air, and, finally—the greatest achievement of all—they make converts of two Fellows of the Royal Society!! The climax of absurdity is here surely reached. Can even spiritualism and human credulity go further? is there no limit to either? Are we destined to see the revered letters F.R.S. lowered to mean nothing more than "Follower of Rampagious Spiritualism," or are we to believe that spiritualism, with all its tomfoolery, its absurdity, and its outrageous defiance of both common sense and common decency, really does contain some modicum of true science, and that there is

More in heaven than is dreamt of in our philosophy?

Are we to believe that the trained intellects of Mr. Crookes, F.R.S., and Dr. Huggins, F.R.S., aided by the refreshing innocence of Mr. Home, have discovered a new and subtle force in nature, which, under the mystic title of "Psychic Force," is not only to revolutionize Nature herself, but to undo all the past and accepted teachings of science? For this "psychic force" is no child's play, it is a force of so powerful a character that no laws of science can stand before it, it falsifies every law in Physics and Biology, it creates force, it despises gravity, it can create music, and bid an accordion play when no one is touching it, and yet it is so obedient that it does all this, and much more than this, at the bidding of one man! And who is this great, this most powerful of human beings, a man who, apparently at will, can directly create force, can

annihilate gravity, and can cause "sad and plaintive" music at his bidding? And the answer comes to us, one Mr. Daniel Douglas Home. As we bow before him, in reverent submission to a superior being, memories crowd upon us; surely we have heard of him before. No man of true genius in this enlightened country can keep his light long under a bushel, and a man of his transcendent power could not possibly do so. Is it not he whose mighty works made the readers of the "Cornhill Magazine" in 1860, tremble as they read? is it not he who in that memorable essay, "Stranger than Fiction," was the medium at whose bidding tables and chairs became imbued with a strange superabundance of animal spirits—when accordions played in distant corners, and then, for mere variety, floated in the air, and there played such marvellous melodies that they rang in the ears of those who heard them—was it not Mr. Home who floated about the darkened room, and made his mark upon the ceiling, and performed other marvels still more strange—surely, there cannot be two men in a lifetime endowed with such miraculous and mystic powers? Is it not the same Mr. Home, who since that time has been the pet of society, the great medium, the associate of crowned heads, and the persecuted victim of the law, who now at last has performed experiments which have caused two Fellows of the Royal Society to burst upon an astonished world with this terrible, this new, this revolutionary "Psychic Force"?

Such is the subtle power of this mighty and powerful man that at his bidding two gentlemen, men distinguished for their scientific attainments, both Fellows of the Royal Society, have surrendered their usually calm and sound judgments, and have themselves borne testimony that even practised men of science may be victims to that most false of all beliefs, that "seeing *is* believing." Surely one would have thought that men who are conversant, or should be conversant, with all the intricate details of many physical phenomena, of the trickery which can be so easily produced by electrical and optical arrangements—trickery so hard and so difficult for the unscientific mind to understand—that such men would be the last to make shipwreck of their reputations on the result of such experiments as they have done. Surely they of all men should know or ought to know that in such experiments the eyes are the most deceptive organs possible, and yet these are the very men who come forward and in the pages of the last number of the "Quarterly Journal of Science" show that they have allowed their judgments to be deceived as well as their eyes.

When Mr. Crookes, soon after he undertook the editorship of the "Quarterly Journal of Science," published an article (now a year ago) announcing his intention of testing spiritualism

by scientific methods and scientific research, he had the sympathies of the majority of scientific men with him, and it was hoped that soon we should have the fallacies of this last and grossest form of imposture exposed and annihilated. It was fondly imagined that, just as the spiritualistic humbug of the Davenport Brothers was shown to be but clever conjuring, so the audacity of the claims of spiritualism would receive a well-directed and crushing blow at the hands of Mr. Crookes. Great is, therefore, the disappointment of the scientific world to find, after all the blowing of trumpets which heralded these investigations, that now Mr. Crookes and his coadjutor, Dr. Huggins, have allowed themselves to be convinced by the results of *two* experiments only, performed one evening in the house of the former gentleman. And such experiments! We almost blush as we read them, to think that scientific accuracy is so degenerate now-a-days in England that any persons, much less men of science, could, by any stretching of a most vivid imagination, venture to call *them* scientific. For in truth they are the very opposite of scientific. Even to call them unscientific is not strong enough; clumsy and futile are much nearer the truth. Imagine men of science deliberately investigating the music-producing powers of an accordion *in a wire cage placed under a dining-room table!* Imagine also when they confessedly had suspicion of Mr. Home's honesty, and one of them watched him dressing to see that he concealed no apparatus about his person, that they placed the accordion in his hands before placing it in the cage under the table! But properly to understand the futile character of these so much vaunted experiments it is only necessary to take the experiments in detail and criticise them.

In the first place, then, there are only *two* experiments described—a number grossly insufficient to cause any reliance to be placed on their results. Errors of observation, errors in the mechanical arrangement of the apparatus are sure to arise, and it is only by repeating such experiments that these errors can be detected and allowed for. Who could have imagined any scientific men describing a "new force" on the results of but two crucial experiments, however many trial experiments, of which we are told nothing, may have preceded them? And then the "crucial experiments" themselves—what are they? For the first experiment the following was the apparatus prepared:—"A cage was formed of two wooden hoops, respectively one foot ten inches and two feet in diameter, connected together by twelve narrow laths, each one foot ten inches long, so as to form a drum-shaped frame, open at the top and bottom. Round this fifty yards of insulated copper wire were wound in twenty-four rounds, each being rather less than an inch from

its neighbour. These horizontal strands of wire were then netted together firmly with string, so as to form meshes rather less than two inches long by one inch high." Then, after all this care and trouble in preparation, this ingenious cage was not placed openly in the room, nor in any conspicuous place, but, to such a low level is scientific accuracy now reduced, that it was actually placed *under the dining-room table.* No reason for this strangest of all strange positions is even hinted at, and can anyone cognisant of the meaning of true scientific research believe that he is reading of the experiments of scientific men, and those men Fellows of the Royal Society?

But to proceed. The apparatus being arranged, Mr. Home entered the room (one, by the way, which he is thoroughly acquainted with, as he had been present before "on several occasions"), his toilet having been watched by Mr. Crookes, who, strange to say, seems to have believed him capable of "secreting machinery, apparatus, or contrivances about his person." We are not aware that even the most famous conjurors conceal apparatus about their person of such great size that anyone casually sitting in their bedroom whilst they are dressing could not fail to detect it—but then we must live and learn! Mr. Home then sat down at the side of the table, with the cage between his legs, and taking the new accordion (specially purchased for this occasion) in his left hand between the thumb and middle finger, at the opposite end to the keys; the bass key having been opened, it was placed in the cage with the keys downwards, and the cage was then pushed under the table as far as Mr. Home's arm would permit, so that his hand was visible. Then the performances—we beg pardon—the "scientific and crucial investigations" began. As a scientific preliminary we are kindly told that the temperature of the room—a most necessary fact obviously to know—"varied from 68° to 70° Fahr."! "The accordion was soon seen waving about in a somewhat curious manner, then sounds came from it, and finally several notes were played in succession," and whilst this was going on Mr. Crookes' assistant most obligingly looked under the table, and reported Mr. Home's hand "quite still," his right hand resting on the table. From this promising beginning we are quite prepared to be told that "presently the accordion was seen oscillating and going round and round the cage and playing at the same time," and it was now Dr. Huggins' turn to look under the table, and again Mr. Home's hand was "quite still, whilst the accordion was moving about and emitting distinct sounds." Were we dealing with really scientific experiments and not such as we are describing, we would ask how it was possible for Mr. Home's hand to hold this moving accordion and yet be

"quite still," but as we are dealing with this peculiar "Psychic Force" we refrain and pass on.

The accordion then proceeded to greater lengths, and, after some preliminary note sounding, played a "simple air," and later on "played chords and runs, and afterwards a well-known, sweet, and plaintive melody, which it executed perfectly, in a very beautiful manner"! and during this performance, it being now Mr. Crookes' turn, he felt Mr. Home's arm and hand, and again, of course, "he was not moving a muscle." Mr. Crookes seems to have had a lurking suspicion that the gifted Mr. Home was capable of playing the accordion with his boots, for he states that during these performances he had Mr. Home's feet held, no doubt by his obliging assistant. Mr. Crookes again was apparently so impressed by the cares of mounting guard over Mr. Home's person, and so enthralled by the abundant music the accordion poured forth, that it never occurred to him to notice *whether the keys were depressed or not*; and yet, to most people gifted with a little common sense, it would be obvious that if the keys were not pressed down, it was impossible for the music really to have come from the accordion, and its *true source* must have been looked for elsewhere. One who risks his scientific reputation on such carelessly performed experiments as these will, we can readily credit, believe anything; and so we are not surprised to read that "Mr. Home actually let go the accordion, and removed his hand quite out of the cage, and placed it in the hand of the person next him, *the instrument then continuing to play whilst no one was touching it!* And this even is surpassed by the next paragraph, the meaning of the first words of which, we are bound to confess, has completely baffled us. "The accordion *was now again taken*, without any visible touch, from Mr. Home's hand, which he removed from it entirely; I and two of the others present not only seeing his released hand, but *the accordion also floating about with no visible support* inside the cage;" and this "was repeated a second time after a short interval." (The italics are our own.) Now, what are we to understand by the words "the accordion was now again taken"? Taken by whom?—by what? The whole paragraph is the most wonderful one it has ever been our lot to read, especially when we consider it is written by a "scientific investigator." The one redeeming feature about it is, that at last we have reached the limits of Dr. Huggins' credulity; he writes to say that *he* did *not see* the accordion freely suspended in air, and at the same time he continues, "I express no opinion as to the cause of the phenomena which took place." We hope he will pardon us for saying so, but it would have been better for his scientific reputation had he made use of this same caution at the beginning, and not at the end, of these pseudo-scientific experiments.

Throughout the whole of these experiments we are left in the most glorious uncertainty as to time: we have not the faintest idea given us how long the experiments lasted, how long the accordion was playing its music, how long it contravened the laws of gravity, and floated about in the air of a cage placed under a dining-room table. The greatest obscurity is thrown over all, and yet we are gravely told this is a "scientific and crucial experiment"! Supposing now for the moment (and this is pure supposition only), that Mr. Home were capable of trickery, and that Mr. Crookes and Dr. Huggins were capable of being deceived; and suppose that a thin wire or thread was hooked on to each end of the accordion, and that a concealed and prepared musical box or other instrument was played, what is there in these experiments unaccounted for? * But we immediately banish these base suppositions from our mind, for we are recalled to a sense of our errors by remembering that we are writing as if trickery and deception were possible in the presence of two Fellows of the Royal Society, gravely engaged in "a scientific investigation of a new force."

Probably by this time our readers have had enough of experiment No. 1; let us now turn to experiment No. 2, which is of an entirely different kind, and one designed, as we are informed, to test Mr. Home's powers of causing "alteration in the weight of bodies," a power which Mr. Crookes states is "most striking, and most easily tested with scientific accuracy." With him here we most cordially agree, so long as the proper apparatus is used; but not under the conditions which Mr. Crookes considered sufficient. The apparatus which he employed was as follows:—"It consisted of a mahogany board 36 in. long by 9½ in. wide and 1 in. thick. At each end a strip of mahogany 1½ in. wide was screwed on, forming feet. One end of the board rested on a firm table, whilst the other end was supported by a spring balance hanging from a substantial tripod stand. The balance was fitted with a self-registering index, in such a manner that it would record the maximum weight indicated by the pointer. The apparatus was adjusted so that the mahogany board was horizontal, its foot resting flat on the support; and in this position its weight was 3 lbs., as marked by the pointer of the balance." On Mr. Home placing his fingers lightly on the extreme end of this board, the pointer of the balance descended, and after a few seconds it rose again, and this movement was repeated several times, "as if by successive waves of the Psychic Force"! At the end of these experiments, the pointer had marked a maximum fall of 6 lbs.

* We would note that it is always *an accordion* that is played at these *séances*, never a concertina or any other instrument.

Mr. Home exerted his pressure at a distance not more than 1½ inches from the extreme end; and since the wooden foot was 1½ inches wide and rested flat upon the table, Mr. Crookes shows, and shows rightly, that no amount of pressure within that space could produce any action on the balance. The whole value of this experiment turns upon one fact, the utter immovability of the table itself; and, as we might have anticipated, this is the one precaution not attended to. The slightest examination of the apparatus will show that if the table moved in any so slight a degree, the index of the balance must descend, and successive movements of the table would produce exactly the same effect as "the successive waves of the Psychic Force." We leave it to our readers to imagine which would be the easiest way to account for the results produced, and especially when they recollect how the mere touch of Mr. Home's hand caused so much motion in the accordion, and then think how the table might have to suffer in the same manner.

Mr. Crookes takes credit to himself for having given "a plain unvarnished account" of his experiments, and we have but faithfully quoted him. It is a true maxim, "The greater the pretensions, the greater the failure," and a more lamentable exhibition of misdirected energies it has never been our lot to read of. But, worst of all, by far the most damaging fact throughout is to publish it to the world that these are samples of *scientific* experiments. These—in which every minute detail, and all obvious precautions, are neglected, where caution should be the rule and is the exception—where the greatest possible care should have been shown, but where all is most careless and most untrustworthy—are such experiments to be called scientific, because, forsooth, an F.R.S. is the investigator?

And then, again, the conclusion of this remarkable account, the maudlin complaints that real men of science had neglected this question, had refused to entertain it, and would not consent to act on committees to investigate it—what is all this but the repetition of what we have so often heard and always know to be untrue. Mr. Crookes *was* solicited to repeat his experiments, or any one of them, at the last meeting of the British Association at Edinburgh, and Professor Stokes even went so far as to propose the appointment of a committee of Section A; but as usual, of course, nothing came of it—the subject was wisely dropped. Some experiments, like some stories, won't bear too much investigation. What was the result of the St. Petersburg committee of six scientific men appointed to investigate Mr. Home's spiritualistic pretensions? To Mr. Crookes the explanation of the failure, he says, "appears quite simple," "Mr. Home's power is very variable and at times entirely

absent;" "when the Russian experiment was tried it was at a minimum," and so on. The real facts of the case will, we think, form a fitting commentary on the experiments we have been criticising, and will also show how far Mr. Crookes is entitled to be considered an impartial scientific investigator. We quote from the "Russian Academical Gazette." "*A glass table* was employed, on which stood a lamp with a reflector, so that the ground underneath the table was brilliantly illuminated and the slightest movements made by Mr. Home could be observed. When the *séance* had begun, Mr. Home announced that he began to feel the presence of spirits, and that these were manifesting themselves by the fluctuations of the flame of a taper standing on the table. To this it was replied that these fluctuations were produced not by spirits but by the ventilator; in fact, when this was shut, the fluctuations ceased. He then said the quick throbbing of his pulse showed the presence of spirits, but one of the committee proved that this was only due to excitement and fatigue, since his pulse beat as fast as Mr. Home's. After these two failures the medium gave up the experiment with the table and proposed to alter the weight of some object. A common bucket was then placed on a weighing machine. The company waited long and in vain; no change of weight occurred. When, finally, the *séance* broke up, and nothing whatever had been accomplished, Mr. Home promised to repeat the experiment; but next day he gave out that he was indisposed and therefore unable to keep his engagement." Surely it required no F.R.S. to tell us that on this occasion Mr. Home's power was at a minimum; most candid readers would confess he had none at all.

And now, finally, a word in conclusion. Until Mr. Home and his friends and allies Mr. Crookes and Dr. Huggins and "the well-known serjeant-at-law," Serjeant Cox, whose scientific aspirations have led him to play the part of the Chorus in a Greek play—until these gentlemen will submit their mystic forces, whether spiritual or "psychic," to a most searching and public scientific examination, they cannot hope to meet with any credit, either for honesty of belief or for scientific accuracy. Truth ever is and ever should be above suspicion; it may lie at the bottom of a well, but we are sadly mistaken if it does at the bottom of a wire-work cage. And when we say that we much prefer everything above-board, we certainly make no exception when that board is a dining-room table.

# THE MOSS WORLD.

By R. BRAITHWAITE, M.D., F.L.S.

[PLATE LXXVII.]

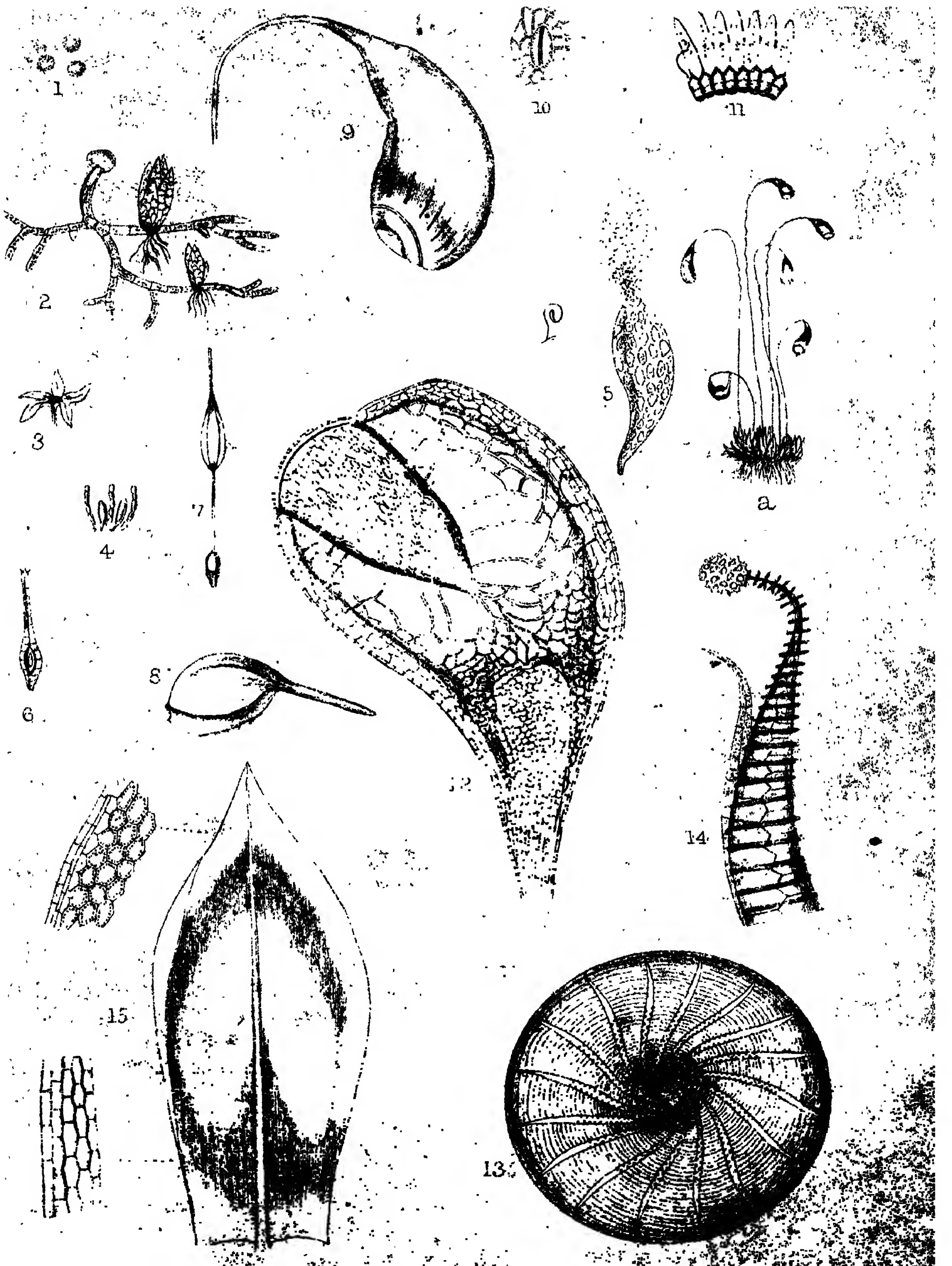
"Muscus in parietes hærens, contemptim a multis adspicitur, at iis, qui ad examen structuræ et virium descendunt, infinitæ admirationis occasionem præbet."—HEINZIUS.

MOST active students of British Botany arrive at a period when having mastered the flowering plants and ferns, they look round for some new field for investigation, in doubt which to "take up" of the four great groups of cellular plants still remaining, the Mosses, Lichens, Algæ or Fungi; each a world within itself, and offering material sufficient to occupy the longest life. Be it our task on the present occasion to say a few words on the first of these, and thus haply open out a new store of enjoyment to some who, hitherto, have not directed attention to them.

By the ancients, as by the unlearned of the present day, mosses were but little regarded, few species were distinguished, and with them were confounded various lichens and algæ. Now, however, that the microscope is in such general use, there can be little difficulty in referring any cryptogamous plants to their proper class, and none has its characters better defined than that of mosses. The first publication which brought them specially into notice was the "Historica Muscorum" of Dillenius, published at Oxford in 1741, in which the species are carefully figured, with enlarged outlines of the leaves; then came (1778–94) the various works of John Hedwig, in which were made known the whole process of their reproduction and their anatomy, and thus Bryology was established on a sound basis; following him came Bridel, Schwaegrichen, Hooker, and a host of other writers down to C. Muller, Wilson, Schimper, Lindberg, and Mitten of our own time, by whose publications the study has gradually been perfected.

The moss world, however, includes among its citizens forms so diverse in habit and structure, that three great groups are at once recognisable.

1. BRYINÆ, the frondose or true mosses, embracing probably




R. Braithwaite del.

W. W. & Co. imp.

10,000 species, of which about 550 enter into the British Flora. These have spores free from spiral threads, which produce on germination a branched confervoid prothallium, the leaves undivided, consisting of uniform cells, the branches dichotomous or pinnate, the capsule opening usually by a lid and enclosing a spore sac.

2. SPHAGNINÆ, the Bogmosses, a limited group, having spores without spiral threads, which produce on germination a lichenoid prothallium, the leaves composed of two kinds of cells, the larger perforated and containing spiral fibres, the branches in lateral bundles of three to ten, the capsule sessile on a lateral naked branch.

3. HEPATICINÆ, the Liverworts, have spores with which are mixed elaters or spiral threads, producing a lichenoid prothallium, leaves nerveless, often lobed and furnished with stipules, or absent, and the whole plant resembling the thallus of a lichen; capsule without operculum splitting into four separate pieces, or sometimes two-valved, or clustered on a receptacle and opening by teeth.

We shall on the present occasion confine our attention solely to the frondose mosses, and pass in review the various organs revealed to us by the microscope. They have been called flowerless plants, but incorrectly, for we shall see that the reproductive organs though minute are very distinct, and their function is unmistakable; they are natives of every clime, and from sea-shore to the limit of perpetual snow are everywhere distributed, preferring, however, mountainous and woodland districts, because there is moisture most abundant; and for this reason also we find them most luxuriant in winter and spring, thus compensating for the absence of the more highly organised plants, which no sooner cover the surface of the earth than our little mosses are no longer noticed.

The denizens of the moss world are never found leading a solitary existence, but are either gregarious or more commonly densely aggregated into tufts or mats, which may contain hundreds of individuals; their size also is very variable, some species of *Ephemerum* not exceeding $\frac{1}{12}$ inch in height, while *Polytrichum commune* and some Hypnoid mosses attain a foot. We will take as our type the cosmopolitan *Funaria hygrometrica*, never absent from the ballast of our railway-banks, brick-fields, and heaths; and having carried home a tuft, let us expand it in water and detach a single plant.

## VEGETATIVE SYSTEM.

First, we observe our Funaria has roots like all other mosses, and these probably not so much required for absorption as in

higher plants, but rather as a means of fixing the plants to the substratum on which they grow, for we find them on rocks and trunks of trees, where their chief support must be derived from the atmosphere; in very many species they occur not only at the base but also along the whole stem; sometimes these adventitious radicles are so dense that they mat the plants together into a spongy mass, and thus effectually retain a supply of moisture; the microscope shows us that each radicular fibril consists of a single series of cells, the transverse partitions of which are oblique. Then we see a short stem bearing leaves, with which also all mosses are supplied, and in this instance the fruit-stalk is a continuation of the stem-axis or is acrocarpic, any continued growth being by innovations or lateral repetitions of the stem; but in many mosses the fruit is lateral or pleurocarpic, and lateral branches are produced through a succession of years.

The leaves are attached horizontally, and are always sessile and persistent, their true arrangement being spiral; in Fissidens distichous or in two opposite rows ($\frac{1}{2}$) i.e. one spiral turn containing two leaves, in some tristichous ($\frac{2}{3}$) two spiral turns cutting through three leaves, and often five or eight rowed ($\frac{2}{5}$ $\frac{3}{8}$).

If we now tear off a few leaves from our Funaria, and place them in water between two slides and transfer to the microscope, we may learn a great deal about moss structure. First, as to the form of the leaf; this we observe is ovate, with the apex pointed, and the ovate or lanceolate form is the most common among the leaves of mosses, though we find every degree of expansion between orbicular and awl-shaped. Traversing the centre from base to apex is a midrib or nerve, composed of several layers of narrow cells; this in some species is altogether absent, in very many it vanishes about the middle or two-thirds the length of the leaf, while in others it is excurrent or extends beyond the leaf in a point, or it may be prolonged into a bristle or hair, and these by their number give the tufts of plants a woolly or hoary aspect. The margin is entire, but in many species it is variously toothed or serrated, and sometimes it has a thickened border. The lamina, or expanded plane of the leaf, is we observe composed of cells, by which a network is produced termed the areolation, and so constant are the individual cells in form and size, that a careful study of them is of the utmost importance in the identification of species, and indeed our only means of determining them when in a barren state.

In Funaria we find the surface is quite smooth, but in many species it is covered with papillæ; in none perhaps are these more evident than in the leaf of *Thuidium tamariscinum*,

which thus resembles a rasp. In form the cells exhibit much variation, but they are referable to two types,

1. PARENCHYMATOUS when the ends are flattened, and we get a quadrate or hexagonal areolation as in Funaria; and sometimes the cell walls are so thickened by internal deposit that their cavities appear like dots, as we see in Grimmia, Orthotrichum, Andreæa, etc.

2. PROSENCHYMATOUS when the transverse walls of the cells are oblique, so that the cells have pointed ends, as in Bryum, Hypnum, &c.

According to the quantity and tint of the chlorophyl contained in the cells will be the shade of colour of the plants; this is usually more or less green, but in Andreæa it is chocolate-brown or black, and some mosses are not unfrequently tinged with rosy purple or brown. The cells forming the leaf base are often of a different form from the upper cells, and when the leaves are closely imbricated they are thinner and usually empty; but those situated at the outer angles are sometimes very different from the rest, and then these alar cells become of value in the distinction of species and even of genera, as in Dicranum, for instance.

### REPRODUCTIVE SYSTEM.

If we hunt up Funaria in December we shall find that instead of the long-stalked capsules the plants bear little rosettes of leaves, which are truly the flowers, and contain in their centre those reproductive organs, which even more clearly than stamens and pistils are necessary for the formation of fruit. The little starry heads at the end of the young lateral shoots are the male flowers, and by dissecting away the enveloping perigonial leaves in water we arrive at a cluster of fine-jointed threads or paraphyses, among which are little sausage-shaped bodies, the antheridia, which at maturity give out a fluid containing excessively minute spiral threads, the spermatozoids; and it is to preserve the vitality of these that paraphyses are present, so as to keep up a certain amount of moisture; for the paraphyses are wanting in the closed bud-like flowers of Hypnum. If we look among the leaves terminating the stem we shall find similar female flowers also containing paraphyses, and longer and more slender bodies, archegonia, which are traversed by a fine channel, and open at maturity by a trumpet-shaped orifice, into which the spermatozoids pass, and reaching the germ cell in the base of the archegonium, this becomes fertilized and at once begins to develop into fruit.

In some mosses these two kinds of organs exist in one flower,

in others, as Funaria, the flowers are separate but on one plant, while in others they are on separate individuals; these three modifications constitute the synoicous, monoicous, and dioicous inflorescence.

As the germinative cell enlarges it pushes its way downward into the receptacle, and then increasing in diameter ruptures the membranous envelope of the archegonium, which is carried up on the rising fruit-stalk and becomes the calyptra, while an extension of the receptacle sheathes its base and is termed the vaginula. When the fruit-stalk has attained its full length its apex begins to enlarge and rapidly becomes moulded into the future fruit; if, when this is fully formed but still green, we make a vertical section of it, we see that the centre is occupied by a longitudinal bundle, the columella, between which and the capsule wall lies the sporangium or spore sac. The inner wall of the spore sac is usually adherent to the columella, and its outer to the inner wall of the capsule, but both may be free as in Polytrichum, or the sporangium may be suspended from the capsule wall by threads as we see in Funaria, in which also the columella is very thick, and occupies a large portion of the cavity of the sporangium.

## THE FRUIT.

The mature capsule or theca of mosses is at once the most striking and elegant part in these little plants, and never fails to excite the admiration of all who deign to notice them; the most elegant forms, adorned with the richest colours, are seen in the various groups, and these are still further enriched by the marvellous beauty often seen in the peristomes.

The thin membranous calyptra, often slit at the side by the distension of the fruit, is first thrown off, and we see the perfect capsule, furnished in most cases with an operculum or lid closing its mouth. The surface of the capsule is smooth and frequently furnished with stomata, well seen in the green fruit of Funaria; the cells composing the outer wall are of firm texture, and in Orthotricum and others in which striæ occur, these are formed of different shaped cells. In Splachnaceæ the neck of the capsule is swollen out into what is termed an apophysis. The forms of the capsule are infinitely varied, but spherical, ovate, pyriform and cylindric, are of most frequent occurrence.

The operculum also presents various forms; in Funaria it is only a little convex, in many it is conical or obliquely rostrate, and its figure is very constant in individual species. Very frequently there is interposed between the mouth of the cap-

sule and the lid an elastic ring, the annulus, and in none is it more evident than in Funaria hygrometrica, where its rich purple colour contrasts remarkably with the parts adjoining. The cells of which it consists are vesicular, and form several series; these contract in drying and are suddenly dilated when moistened, and thus the separation of the operculum is greatly facilitated. The lid having fallen, there is now brought into view in most mosses a beautiful fringe of teeth called the peristome, which originates from the inner wall of the capsule, and is most remarkable for the definite number of its constituent parts, 4, 8, 16, 32 or 64; frequently also an inner peristome is present, continued from the outer membrane of the spore sac.

If we take the most perfect form of teeth, as seen in Mnium, Hypnum, or Funaria, we find that each consists of two rows of firm coloured cells, separated in the middle by a divisural line, and also articulated with each other transversely; on the inner face there is only one row of cells, and these frequently project at the margin beyond the outer layer, and also form transverse lamellæ at their junction; their texture is also quite different from those of the outside layer, for they are pale and vesicular, and on them the hygroscopic property of the peristome depends. When moistened these internal cells expand, and thus shorten the teeth and draw them inward into a cone, their usual position in the dry state being radiating round the mouth of the capsule or reflexed against its outer wall.

Numerous modifications of the teeth occur, which render the peristome of interest as a microscopic object, the inner layer of cells being frequently abortive, the teeth become rigid, and sometimes quite rudimentary, and not unfrequently they disappear altogether, the capsule being then termed gymnostomous. In Funaria the teeth are curiously gyrate, and also attached to an unusual appendage in the form of a little perforated central disc, to which the points curve down, so that the part not inaptly resembles the inverted bottom of a wine-bottle; in Tortula they are twisted together spirally, and sometimes united at base into a tubiform membrane; in Dicranum and Fissidens they are cleft along the divisional line, and in Grimmia apocarpa perforated or cribrose. Two other forms of peristome, however, are met with, in which the structure of the teeth is essentially different. In Georgia and Tetrodontium the four teeth consist of numerous confluent cells, and in Polytrichaceæ the numerous teeth are composed of slender filaments agglutinated together into tongue-shaped processes, which adhere to the tympanum or expanded apex of the columella; in the Australian genus Dawsonia the filaments are free, and project from the capsule like a brush. These three

modifications of the peristome are advantageously employed by Mr. Mitten for divisions of the order.

The inner peristome is quite different from the outer, and consists of a thin membrane arising from the outer wall of the spore sac, and divided into processes which stand opposite the interspaces of the outer teeth; in its most perfect form these processes are sixteen in number, each two cells wide at base, projecting outwardly along the middle line as a keel, or perforated and cohering only by the transverse articulations of the cells; between each pair of processes are three very fine cilia each one cell wide, so that the whole circuit of the inner peristome takes up eighty cells.

This fully-developed, internal peristome is well seen in the genus Mnium, and in Plagiothecium undulatum, Hypnum riparium, commutatum, &c.; but in many mosses with a double peristome the cilia are abortive, and the processes also become quite rudimentary. The columella is a continuation of the central part of the pedicel, extending to the operculum, with which it often comes away; it is commonly united to the wall of the spore sac, but is wanting in Archidium and Ephemerum; in the Polytrichaceæ its summit is dilated into a membrane like a drum-head, which closes the mouth of the capsule.

The spores are globose or kidney-shaped, smooth on the surface or rough with papillæ, and in colour yellow, red, or ochreous; in Archidium very large and few in number, but in the majority of mosses they are extremely minute.

## Propagation.

Having thus briefly glanced at the various organs found in mosses, we may notice the mode by which their growth and continuance is provided for.

The spore does not (like the seed of a flowering plant) produce an individual like its parent, but on rupturing its outer coat, the primordial utricle is protruded as a proembryo, and commences a process of cell division, resulting in a confervoid filament, which gives off lateral branches and branchlets, forming the delicate green film we may often notice on damp earth. From various cells of this prothallium young plants are developed, fine radicles push downward, and true leaves are formed characteristic of the species, numerous plants thus resulting from a single spore; and when this rising generation is able to take care of itself, the prothallium disappears, except in a few annual species constituting the genus Ephemerum, where it continues throughout the life of the plant. Many mosses, however, are met with which do not produce fruit, yet the

continuation of their existence is singularly cared for. In Aulacomnium and other mosses we constantly find in place of fruit a pseudopodium or naked stalk bearing at the summit a globose head of gemmæ, and in Orthotrichum Lyellii and other species the leaves produce on the surface jointed filaments, and both these appendages on falling off develope prothallium, from which new plants originate. The same takes place with tubercles which develope on roots, and thus we may strip the rocks of their tenants Grimmia, Rhacomitrium, Tortula, &c.; yet a few years suffice to restore them from the adhering radicles left behind; and again the caducous ramuli of Campylopus and Leucobryum become new plants, while even a single deciduous leaf of Funaria hygrometrica has been seen to produce prothallium from its basal cells, by which the establishment of a new colony is secured.

### Classification.

The arrangement of mosses is confessedly a subject of great difficulty, and one that has occupied the thoughtful attention of all Bryological writers. Up to a recent date the peristome alone was used as the chief character by which to establish genera, and thus species were thrown together which agreed in nothing but the number and form of the teeth in this organ; it may be interesting, however, to give an outline of the principal systems that have been proposed.

1. Hedwig published the first of these in his "Fundamenta Muscorum," 1782, with 25 genera arranged in 3 divisions. (1) Without a peristome (*Phascum*). (2) With a naked peristome (*Sphagnum*, *Hedwigia*, *Gymnostomum*). (3) With a fully-formed peristome, sub-divided into: *a*. Peristome single (11 genera). *b*. Peristome double (10 genera).

2. Bridel, in the "Bryologia Universa," 1826, greatly increased the number of genera, arranged as follows:—

Sec. 1. *Capsule operculate.*

Class 1. Fruit without a vaginula on a pseudopodium (*Archidium*, *Sphagnum*). Class 2. Fruit acrocarpous. Class 3. Fruit pleurocarpous. Class 4. Fruit radical. Class 5. Fruit axillary. Class 6. Fruit under an accessory leaf.

Sec. 2. *Capsule cleft* (Andreæa).

3. The next important alteration in system was that adopted by C. Müller, in his "Synopsis Muscorum," 1849, in which the minute structure of the leaf takes a prominent part in the characters of tribes and genera, and many gymnostomous species

are made congeneric with others having a peristome, but with which they otherwise agree in habit and leaf-structure; e.g., Gymnostomum is abolished, and its species distributed under Weisia and Pottia; his principal divisions are:

Class 1. *Schistocarpi* (Andreæa).
Class 2. *Cleistocarpi* (the old genus Phascum).
Class 3. *Stegocarpi.*

Sub-class 1. Acrocarpi.
Sec. 1. Distichophylla.
Sec. 2. Polystichophylla.
Sec. *a.* Leaves not papillose.
Sec. *b.* Leaves papillose.

Sub-class 2. Pleurocarpi.

4. This was still further elaborated by Professor Schimper, in his valuable "Bryologia Europæa and Synopsis Musc. Europ.," 1860, by the formation of additional tribes and families and very many genera, some indeed on characters which do not appear to be sufficiently important.

5. Mr. Mitten, in his descriptions of Indian and South American Mosses in the "Journal of the Linnean Society," proposed a new system, which, notwithstanding its great merit, does not seem to have met with general acceptance, probably because there still lingered a strong bias in favour of Hedwig and his successors. He places all mosses under two sections: Homodictyi, having leaf cells of uniform structure, and Heterodictyi, having leaf cells of two forms and including only the Sphagnaceæ. The Homodictyi again have two divisions Schistocarpi and Stegocarpi, and the latter comprises three groups according to the structure of the peristomial teeth already described. The families also are reduced and thus brought more in accordance with the natural orders of flowering plants; while the leaf structure receives its due importance the peristome is regarded as subordinate, every degree of its development being found in a single genus, and sometimes in a single species. Believing these views to be strictly in accordance with facts derived from a careful study of the plants themselves, and therefore true to nature, I feel bound to adopt it, though I have ventured to deviate a little from the arrangement, believing that the retention of the acrocarpous and pleurocarpous sections is certainly convenient.

## TABLE OF THE FAMILIES OF MOSSES.

Subclass.—*SPHAGNINÆ.*

Fam. 1.—SPHAGNACEÆ.

Subclass.—*BRYINÆ.*

Order 1.—SCHISTOCARPI.

Fam. 2.—ANDREÆACEÆ.

Order 2.—STEGOCARPI.

Division 1.—*Elasmodontes.*

Fam. 3.—GEORGIACEÆ.

Division 2.—*Nematodontes.*

Fam. 4.—BUXBAUMIACEÆ.

„ 5.—POLYTRICHACEÆ.

Division 3.—*Arthrodontes.*

Subdivision 1.—Acrocarpici.

| | |
|---|---|
| Fam. 6.—DICRANACEÆ | Fam. 13.—FUNARIACEÆ |
| „ 7.—LEUCOBRYACEÆ | „ 14.—BRYACEÆ |
| „ 8.—TRICHOSTOMACEÆ | „ 15.—MNIACEÆ |
| * „ 9.—CALYMPERACEÆ | „ 16.—BARTRAMIACEÆ |
| „ 10.—GRIMMIACEÆ | „ 17.—SCHISTOSTEGACEÆ |
| „ 11.—ORTROTRICHACEÆ | „ 18.—FISSIDENTACEÆ. |
| „ 12.—SPLACHNACEÆ | |

Subdivision 2.—Pleurocarpici.

| | |
|---|---|
| *Fam. 19.—HYPOPTERYGIACEÆ | Fam. 23.—NECKERACEÆ |
| * „ 20.—RHACOPILACEÆ | * „ 24.—EROPODIACEÆ |
| „ 21.—HOOKERIACEÆ | „ 25.—LESKEACEÆ |
| „ 22.—FONTINALACEÆ | „ 26.—HYPNACEÆ. |

It will be seen that the Cleistocarpi or closed-fruited mosses are omitted, for the only point in common which they possessed was the absence of a separable operculum, yet Schimper, in his "Synopsis," keeps up for them three families and eleven genera; a twelfth genus, Systegium, comprising Phascum crispum and multicapsulare which have a distinct lid though it does not come off, he removes to the stegocarpous section, and to the vicinity of Weisia, from which indeed it is inseparable in any natural arrangement. The most lowly organised are the species of Ephemerum, the babies of the moss world, so

* Not European.

minute as to be scarcely distinguishable, and so little self-dependant, that they never dispense with their supporting prothallium during the whole period of their existence, and the microscope will at once show their affinity to Funaria in the lax parenchymatous areolation and inflated calyptra. Again, Archidium and Pleuridium are equally approximated to Dicranella, while Phascum itself for which we retain the common P. acaulon Lin. as the type is equally close to Pottia among the Trichostomaceæ.

### Habitats.

Few localities are to be found where some moss or other is not to be met with. Even in our largest towns, a few yards of open ground, if neglected for a time, become covered with a crop of mosses originating from spores wafted on every breeze. Bryum argenteum and Ceratodon purpureus occupy the paths, though probably they do not bear fruit, and Tortula muralis takes possession of the bricks and their interstices on the garden walls.

If we extend our walks to the commons around, we find the list of species considerably augmented, and old sandstone walls bear in addition Grimmia pulvinata, with several Tortulæ and Brya. A few mosses are littoral or affect the sea-coast, especially several species of Pottia, with Trichostomum crispulum, brachydontium and littorale, Tortula nitida and cuneifolia.

Again, the chemical nature of the soil or rock materially influences the character of its inhabitants: some are almost confined to chalk, as Seligeria calcarea and paucifolia, Funaria calcarea, several Tortulæ, Weisia tortilis, Thuidium delicatulum and histricosum, &c.; while clay-fields support the various species of Phascum and Ephemerum. Sandstone has Campylostelium, Brachyodus, Tetrodontium, &c.; and granite, slate, or basalt each supports some peculiar species: to the first of these especially are the Andreæas attached. Bogs are rich in certain species, especially of Mniaceæ, with Hypnum cuspidatum, cordifolium, giganteum, fluitans, nitens, &c. Trees are occupied by various species of Orthotrichum, Zygodon, Cryphæa, and Leucodon—some preferring the smooth-barked willows, others the rougher oak and elder; and, again, some confine themselves to the parts near the ground—as Weisia truncicola, Leskea pulvinata and polycarpa, Anomodon viticulosus, &c. As we ascend the mountains, we soon find we have reached the head-quarters of the mosses—the numerous streams and waterfalls, and the frequent mists that saturate the atmosphere with moisture, keep them growing continually,

and frequently impress a marked feature on the landscape, for species of Polytrichum and Rhacomitrium frequently cover extensive areas, to the exclusion of all other forms. Again, certain species—and some of these, too, among the most elegant—flourish luxuriantly in these elevated regions, which we should look for in vain at a lower level, as Splachnaceæ Conostomum, various species of Grimmia and Andreæa, Hypnum Halleri, callichroum, reflexum, &c.; thus everywhere covering the bare places of the earth with a verdant carpet ere yet Spring dare put forth her flowers—clasping in their tender arms the crumbling stone, and smoothing the scarred face and furrowed cheek of the time-worn tower, or clothing the decaying trees with a mantle of green of varied shades, transforming each into a garden where the observing eye may delight to trace a miniature resemblance to the pine-woods, aloes, yuccas, and sedums among higher plants.

## Uses.

The old writers delighted to attribute many uses to mosses, yet, except among the most primitive races, few of them minister directly to the wants of man; the Sphagnums alone form the first origin of peat, which is still largely consumed for fuel. Linnæus tells us that Polytrichum commune was used by the Laplanders for beds, and highly praises it for not harbouring insects or any infectious disease; and this plant is still used in the dales of the north of England for the manufacture of small brooms; Splachnum Wormskioldii also forms the wick for the simple lamp of the Eskimos.

Yet although their services in human economy are so small, in that of Nature how great is their end! The bear lines his winter quarters with a thick bed of Polytrichum; the squirrel and dormouse, and whole tribes of birds, use Hypnums as material for their nests; and if we shake a tuft of moss over paper, we shall find that it harbours a population we little dreamed of—elegant little mollusca feeding among the branches, with tiny beetles and poduræ and curious acari hiding at the roots.

Again, mosses have been termed the pioneers of vegetation, for, being able to establish themselves where little else can maintain a footing, they penetrate with their slender radicles the smallest crevices, and slowly disintegrate the substratum on which they grow, constantly arresting by their interwoven tufts the dust-grains wafted on every breeze, and ever decaying below, ever extending above, they are slowly but surely accumulating material capable of supporting a higher order of

Their vital functions, too, must not be overlooked, for their myriad cells are for ever condensing the moisture of the atmosphere and adding their tribute to every mountain rill, which, borne onward to the ocean, is again returned to them in the mists and snow-wreaths that are their constant attendants.

In winter and early spring they contribute much to the verdant covering of the earth, and to the supply of oxygen afterwards given out by the leaves of higher plants. I have mentioned the vast number of species that people our moss world, all unsurpassed in beauty of structure by any other group; and these are but a fragment of the Creator's handiwork, unnoticed and uncared for by the ordinary passer-by; yet, though yielding neither sustenance for the hungry nor medicine for the sick, there are times when their study will supply both food and physic to the mind.

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## EXPLANATION OF PLATE LXXVII.

a. Funaria hygrometrica, nat. size.
1. Spores.
2. Prothallium and young plants.
3. Male flower.
4. Antheridia and paraphyses.
5. Antheridium discharging spermatozoids, with one of the latter very highly magnified.
6. Fertilised archegonium.
7. Young fruit with vaginula.
8. Calyptra.
9. Perfect fruit before the fall of the lid.
10. One of the stomata from neck of same.
11. Portion of the annulus.
12. Longitudinal section through the fully-formed green fruit, showing the small sporangium, nearly filled by the columella.
13. Peristome.
14. A single tooth of outer and of inner peristome, with the central cribrose disc.
15. A leaf, with the cells constituting its areolation.

# THEORY OF A NERVOUS ETHER.

By DR. RICHARDSON, F.R.S.

IN my course of experimental lectures on medical science delivered during the last winter session, I broached a modification of the old and well-nigh obsolete theory of the existence of a nervous fluid; and I have since reduced to some form, in a published lecture, the ideas I wished to set forth.* It has been curious to me to observe the different lights in which this effort has been viewed by men of different phases of thought and knowledge. Some physicists have accepted that the theory suggests the existence of an intermediate agency between the matter of living bodies and the forces by which the matter is moved—an agency essential to the correct understanding of the relations coexisting between the living matter and force. Others have thought the theory obscure and retrogressive, a kind of retreat into the bosom of Van Helmont, and of those fanciful heroes of Lord Lytton, who, still professing Helmontism as an article of scientific, and I had almost said moral faith, proclaim life to be "a gas." Lastly, certain enthusiastic writers, and, as they call themselves, experimenters, have actually laid hold of the theory to support modern spiritualism, and its idola of the theatre.

To commence with the last of these critics, I need scarcely say, in relation to them, that there is nothing in my mind bearing in the remotest degree on their arguments. I speak only of a veritable material agent, refined, it may be, to the world at large, but actual and substantial: an agent having quality of weight and of volume; an agent susceptible of chemical combination, and thereby of change of physical state and condition; an agent passive in its action, moved always, that is to say, by influences apart from itself, obeying other influences; an agent possessing no initiative power, no *vis*, or *energia naturæ*, but still playing a most important, if not a

* *Medical Times and Gazette*, May 6, 1871.

primary part in the production of the phenomena resulting from the action of the *energia* upon visible matter.

In respect to those who imagine that the theory of the existence of a nervous ether tends to materialise the phenomena of life and of living action, the answer is simple. The theory treats of an assumed material part of the living organism, and has no reference whatever to that more distant or spiritual essence of our nature of which, as yet, no more is known, physically, than of the *energia naturæ* itself. We must all accept that the impulses of men and animals, the volitions, the sympathies, the passions, are manifested by and through the material organism, the matter as a mechanism obeying the force that moves it. The tongue of man that speaks, the hand that gives, takes, strikes, aids, begs; the feet that make progression, and all parts that act, act positively as mechanisms of material character. How they act, in obedience to the impulses that move them, becomes, consequently, a distinct question that may be studied apart from the impulses themselves, and in the theory this is implied. The impulses, I mean, are considered as initial and independent motions, the origin of which forms no part whatever of the theory of a nervous ether.

Of the first order of critics, they alone appreciate the meaning I would attach to the theory of a nervous or animal ether. The idea attempted to be conveyed by the theory is that between the molecules of the matter, solid or fluid, of which the nervous organisms and indeed of which all the organic parts of the body are composed, there exists a refined subtle medium, vaporous or gaseous, which holds the molecules in a condition for motion upon each other, and for arrangement and rearrangement of form; a medium by and through which all motion is conveyed; by and through which the one organ or part of the body is held in communion with the other parts and by and through which the outer living world communicates with the living man: a medium which, being present, enables the phenomena of life to be demonstrated, and which, being universally absent, leaves the body actually dead—in such condition, i.e. that it cannot, by any phenomenon of motion, prove itself to be alive.

I hope I have now made clear what is generally meant by the theory of a nervous ether. But there are yet two other points on which it is essential, for a moment, to dwell. In using the word *ether* I do not necessarily convey the common idea of a body belonging to the chemical family or group called the ethers, or the ethyl series. I use the word ether in its general sense; as meaning a very light vaporous or gaseous matter: I use it, in short, as the astronomer uses it when he speaks of the ether of space, by which he means a subtle but

material medium, the chemical composition of which he has not yet discovered. Again, when I speak of a *nervous* ether, I do not convey that the ether is existent in nervous structure only: I believe, truly, that it is a special part of the nervous organisation; but as nerves pass into all structures that have capacities for movement and sensibilities, so the nervous ether passes into all such parts; and as the nervous ether is, according to my view, a direct product from blood, so we may look upon it as a part of the atmosphere of the blood.

The theory of the existence and influence of a nervous *fluid* is old. The earliest practical neuro-physiologists seized upon it at once as affording the only explanation of many vital phenomena. Willis, who was the leader of modern neuro-physiology, gave the cue, and after him, up to the time of Galvani, every school taught the theory. "There exists," said the masters—"there exists in the nervous system a distinct fluid, a liquid which proceeds from the centres towards and to the extremities of the nervous system. The nervous centres (the brain included) are thus positively glands; they secrete the nervous fluid and pour it out by the nerves. As bile is secreted by the liver and poured forth, so is the nervous fluid secreted by its centres and poured forth by the nerve ducts." Alexander Munro, in 1783, sums up the argument clearly and tersely, to the effect that the nerves are tubes or ducts conveying a fluid secreted in the brain, the cerebellum, and spinal marrow.

To the physiologists from the time of Willis (who lived, by the way, in the reign of Charles II.) and to those who followed him up to the time when Galvani made his first observations on so-called animal electricity (1790) this hypothesis of a nervous fluid sufficed to explain the varied phenomena of nervous function: the fluid was supposed to convey the vibrations of the outer world to the inner centres of the animal body; the fluid governed secretion; the fluid was the channel by which, or rather through which, the volitional powers of the animal were brought to bear on the muscular mechanism.

Nor must it be ignored that the arguments employed in support of the theory were sensible and were supported by experimental facts. "When," argued the maintainers of the theory—"when we cut a nerve across and bring its parts again into contiguity, we do not restore the office of the nerve immediately, nay, the influence of the nerve beyond the incision is generally not restored: when we compress a nerve we produce numbness by the compression; and when, by repeated slight compressions of a nerve, we cause repeated contractions of the muscles fed by the nerve, we prove that the impulse is exerted on matter which admits of being affected *by simple pressure.*"

Still further, the argument of the velocity with which impressions are conveyed from the centres of the nervous system to the muscles, or from the external surfaces back to the centres —an argument which in our times has been advanced as if it were new in science—was studied by these earlier physiologists who urged that if the nerves are constantly filled or charged with fluid, an impulse given to that fluid at the brain may be suddenly communicated to the most distant organ, or the reverse, although the velocity of the fluid be itself very small.

The modern reader will gather from the above that there was great simplicity, great beauty, great force, in the olden theory of a nervous fluid; and if I could invite him to follow me into the subject of structure of nervous matter he might perchance be more convinced that the theory was in a sense true and unassailable. He would certainly wonder why so little is known, at this day, about so important a speculation.

His wonder would, moreover, be most reasonable, for the theory, while it has never been successfully assailed, never been injured a jot by anything that has been said about it, has merely been let drop and forgotten, hidden for a time by the brilliancy of another theory—now well-nigh blazed out—I mean the electrical theory promulgated by Galvani.

The evidence in favour of the existence of an elastic medium pervading the nervous matter and capable of being influenced by simple pressure is all-convincing. When we press a nerve firmly we act on something that is as distinctly under the influence of the pressure as when we press upon a vein and by that means influence the current of blood within the vein. When we freeze a nerve we stop its function altogether, we make it almost like metal in its appearance and physical character, and we can then divide it without communicating the faintest scintillation of sensation to the brain.* In this case the cold has acted like pressure; it has either condensed the nervous matter, and has, by the contraction induced, driven out some agent the presence of which was necessary for the performance of function, or it has condensed the agent itself together with the nerve, so that the condition for sensation is suspended. When we divide a nerve we break a connection, we divide a structure which must be absolutely perfect for conveyance of motion, and with our best skill we cannot sécure that the connection shall be ever again rendered perfect.

Each one of these experimental facts suggests that there exists in the nerve an actual material mobile agent, a something

* This fact is well illustrated in the operation of nerving the horse, under ether spray. The nerve, when exposed, is found superficially frozen, and when frozen more deeply may be cut as if it were a soft metallic wire, dead to the knife as in the nerveless horny hoof below.

more than the solid matter which the eye can see and the finger touch. The question to be considered, is—the *nature* of this agent.

In nervous structure there is, unquestionably, a true nervous fluid, as our predecessors taught. The precise chemical composition of this fluid is not yet well known, the physical characters of it have been little studied. Whether it moves in current we do not know; whether it circulates we do not know; whether it is formed in the centres and passes from them through the nerves, or whether it is formed everywhere where blood enters nerve we do not know. The exact uses of the fluid we do not, consequently, know.

It occurs to my mind, however, that the veritable fluid of nervous matter is not of itself sufficient to act as the subtle medium that connects the outer with the inner universe of man and animal. I think—and this is the modification I suggest of the older theory—there must be another form of matter present during life; a matter which exists in the condition of vapour or gas, which pervades the whole nervous organism, surrounds, as an enveloping atmosphere, each molecule of nervous structure, and is the medium of all motion communicated to or from the nervous centres.

The source of this refined matter, within the body, is, I think, the blood. I look upon it as a vapour distilled from blood, as being persistently formed so long as the blood circulates at the natural temperature, and as being diffused into the nervous matter, to which it gives quality for every function performed by the nervous organisation. In the closed cavities containing nervous structure, the cavities of the skull and spinal column, this gaseous matter, or ether as I have called it, sustains a given requisite tension; in all parts of the nervous structure it surrounds the molecules of nervous matter, separates them from each other, and is yet, between them, a bond and medium of communication.

When it is once fairly presented to the mind that during life there is in the animal body a finely diffused form of matter, a vapour filling every part—and even stored in some parts; a matter constantly renewed by the vital chemistry; a matter as easily disposed of as the breath, after it has served its purpose—a new flood of light breaks on the intelligence. Our own consciousness re-echoes to us the fact. Our experience assures us that between ourselves and the outer world there is, while we live, an intercommunicating bond which connects us with the outer world; which is apart from the gross visible substances we call flesh, bone, brain, blood; which in some way, nevertheless, is connected with both heart and brain and organs of sense; which is made in and within our own organism;

which produced in over quantity oppresses us; which produced in too small quantity is insufficient for our wants; which is renewed by food and by sleep, exhausted by wakefulness and labour; which receives every vibration or motion from without, and lets the same vibrate into us, to be fixed or reflected back; and which conveys the impulse when we will an act and perform it.

It may be urged that in this line of thought is included no more than the theory of the existence of the ether that is supposed to pervade space, the undulating ether of light. It may be said that this universal ether pervades all the organism of the animal body as from without, and as part of every organisation. This view would be Pantheism physically discovered, if it were true. It fails to be true because it would destroy the individuality of every individual being: it fails to be true because it would destroy the individuality of every individual sense. If we did not individually produce, by our own chemistry, the refined essence pervading us; if the essence were diffused through us independently of our eating, drinking, breathing, we should be independent of the earth altogether, endowed with an indestructible physical existence not belonging to us at all, specially, but to the universe at large, and distinct from us; we should, in fact, be as atoms of matter aggregated by attraction into a certain form or mould, and held to the earth by the attraction of the earth, but actually permeated with the ether, as though we floated in an ethereal sea. If we did not individually produce the medium of communication between ourselves and the outer world, if it were produced from without and adapted to one kind of vibration alone, then were fewer senses required than we possess; for, taking two illustrations only—ether of light is not adapted for sound, and yet we hear as well as see; while air, the medium of motion of sound, is not the medium of light, and yet we see and hear.

In the theory therefore I offer the nervous ether is an animal product. In different classes of animals it may differ in physical quality so as to be adapted to the special wants of the animal, but essentially it plays one part in all animals, and is produced, in all, in the same way.

I think I may venture, to some extent, to define the required physical properties of a nervous ether. We may consider it as a gas or vapour, and as having in its elementary construction carbon, hydrogen, and possibly nitrogen: I suspect it is condensable under cold, movable under pressure, diffusible by heat, insoluble in the blood, and holding at the natural temperature of the body a tension requisite for natural function. It is retained, I imagine, for a longer time in cold-blooded animals, after death, than in warm-blooded animals, and

longer in warm-blooded animals that have died in cold than in those that have died in heat. Upon its presence for a considerable time after death in some animals, and for a short time in all animals under favouring conditions, I believe to depend those post-mortem movements of muscles which Haller attributed to the *vis insita* of muscular fibre.

The nervous ether is not, according to my ideal of it, in itself active or an excitant of animal motion in the sense of a force; but it is essential as supplying the conditions by which the motion is rendered possible. It is the conductor, I presume, of all vibrations of heat, of light, of sound, of electrical action, of mechanical friction. It holds the nervous system throughout in perfect tension during perfect states of life. By exercise it is disposed of, and when the demand for it is greater than the supply, its deficiency is indicated by nervous collapse or exhaustion. It accumulates in the nervous centres during sleep, bringing them, if I may so speak, to their due tone, and therewith rousing the muscles to awakening or renewed life. The body, fully renewed by it, presents capacity for motion, fulness of form, life. The body, bereft of it, presents inertia, the configuration of "shrunk death" the evidence of having lost something physical that was in it when it lived.

The theory of a nervous ether comports itself well in respect to the refined mechanism of the senses. When the wave of atmosphere strikes the tympanum or drum of the ear it communicates the vibration to the nervous ether within, and so, by the auditory tract, to the central organ, the brain. When the wave of luminous ether impinges on the condensing retina it communicates the vibration, through the nervous ether, along the optic tract, to the brain. When the picture of an object is put upon the retina, it is looked at where it is put, on the veritable spot where it is focussed, until it evanishes by being withdrawn or shut off from the sense. When an impression is made on the surface of the body, be it made by heat, electricity, or mechanical excitation, it is vibrated through the ether to the centres of the nervous system; and when an impulse from the centres is conveyed to the muscles, it, too, is vibrated through the same medium.

The ether, as I opine, holds the molecules and cells of nervous matter, the ultimate particles of muscles, the corpuscles of blood, and probably the ultimate particles of the fibrine of the blood in a state of mobility; it thus passively counteracts the attraction of cohesion between particles, and prevents rigidity of the flexible or fluid structures of the body so long as they live. Being itself a simple physical agent, the nervous ether is, I apprehend, influenced in the most signal manner by

simple external conditions. It is influenced by variations of heat and cold, is increased in volume by heat, is contracted or condensed by cold; it is influenced by atmospheric pressure; it is influenced by electrical conditions of the air; the inductive effects of electricity on the muscles of living animals are due, as it seems to me, to the disturbance excited by the electrical action upon the animal atmosphere; nay, I conceive, we ourselves are rendered conscious of the changes of external conditions—of heat, of cold, of variations of the barometrical pressure, of electrical storms—by the sensible fluctuations of the atmosphere within us.

Through the nervous ether, itself a gas or vapour, other gases or vapours may readily and quickly diffuse, and by such diffusion may so modify the physical characters of the natural ether as to lead to modifications of nervous function. Thus those vapours which, being diffused into the body, produce benumbing influence—as the vapours of alcohol, chloroform, bichloride of methylene, ethylic ether, and the like—produce their benumbing effects because they are not capable of taking the place of the natural ether into which they diffuse: they interfere, that is to say, with the physical conduction of impressions through what should be the pure atmosphere between the outer and the inner world. A dense cloud in the outer atmosphere shall shut out my view of the sun; a cloud in the inner atmosphere of my optic tract shall produce precisely the same obscurity.

Pain is the result of rapid vibration of the nervous ether; and pain, whether it be called physical or mental, is the same event. The so-called physical pain, that which comes from a blow or a cut, is excessive vibration, more than the brain can receive. The so-called mental pain is excessive vibration carried through the senses to the centres, or excited in the centres and carried to the outlets of the body for relief.

It is, I think, no figure of speech to say that nerves bleed—no figure of speech to affirm the phenomena of nervous exhaustion, of nervous collapse, of nervous strain and of nervous overstrain. Under mental labour or emotion nerves bleed as vessels do—bleed not blood in mass, but the richest product of blood. Under violent shock the whole nervous atmosphere is thrown into vehement vibration, the heart is held fixed by the commotion, and the failure of animal force is followed by sudden and overwhelming prostration. These are all clear physical phenomena. A feeble animal chemistry yields a feeble nervous tension, a powerful chemistry or action produces over-tension.

The infliction of physical pain is followed by the shriek, the sob, the moan, or the hard setting of muscle: the shriek, the

sob, the moan, or the muscular rigor is the echo of the pain; it is more, it is the outlet of the evil, the excess of vibration reflected, diverted, given forth. The infliction of mental pain is followed by tears, sighs, and other varied forms of grief; these are, again, the echoes and the outlets of the evil.

The tension of the nervous ether *generally* may be too high or too low; it may be so *locally*, owing to local changes in the nervous matter it invests and charges. Under undue tension of the brain or cord, both closed firmly in by bony walls, the ether, under sharp excitation, may vibrate as if in a storm, and plunge every muscle under cerebral or spinal control into uncontrolled motion—unconscious convulsion.

Lastly, the nervous ether may be poisoned; it may, I mean, have diffused through it, by simple gaseous diffusion, other gases or vapours derived from without; it may derive from within products of substances swallowed and ingested, or gases of decomposition produced, during disease, in the body itself. But here a field of observation opens relative to the production of some forms of acute and chronic diseases on which I must not enter, were even space at command.

I have tried, and I hope with success, to offer a simple and practical view of a very difficult subject. The philosopher may think the subject void, the public may think it obscure. There are many, I am aware, who will say that although the theory is reasonable it is comparatively worthless until more is known—until, in short, the physical character of the assumed nervous ether is demonstrated and certain definite phenomena are made manifest by its mediation. This criticism, which I should be the first to suggest, I am the last to ignore. I profess only at the present moment to submit a theory; I look to experiment for the trial of the theory, its truth, its falsity; and as it is a theory which experiment can slowly, but in the most striking and solemn manner, truly and faithfully try, I abide the result with leisure and contentment.

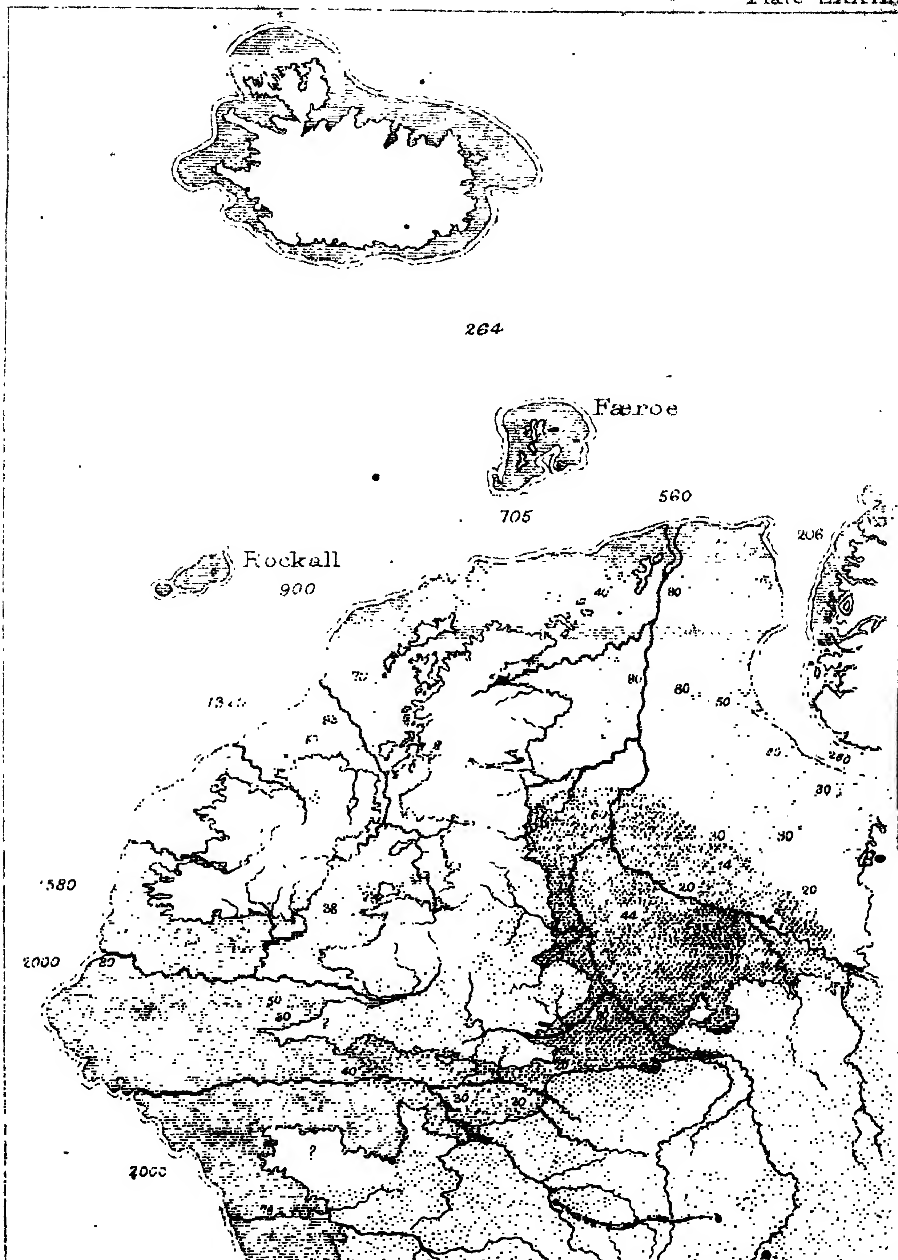
# ON PLEISTOCENE CLIMATE AND THE RELATION OF THE PLEISTOCENE MAMMALIA TO THE GLACIAL PERIOD.

By W. BOYD DAWKINS, M.A., F.R.S., F.G.S.

[PLATE LXXVIII.]

THE animals which inhabited northern France, Germany, and Britain, during the Pleistocene* age, have been defined with the greatest accuracy by M. Lartet, Dr. Falconer, Professor Busk, and others, and probably will not be largely increased by any future discoveries. There are, however, certain inferences to be drawn from the comparison of the present with the former range of the Pleistocene animals, both as to climate and geography, which have not been brought prominently forward. The delicate question also of their relation to the glacial period, which has been *sub judice* for the last twenty years, demands a careful attention. In the following essay I shall attempt to show to what extent it may be solved by an appeal to the localities in Great Britain and Ireland, in which they have been discovered. I have already proved in the "Quarterly Geological Journal" for 1869, p. 192, that the contents of the bone caves and of the river deposits of Great Britain are, zoologically speaking, of the same age, and that the absence of one or two animals in the one or the other does not necessarily imply a difference in point of time. The cave bear is probably absent from the river beds because it was an inhabitant of caves, and was not liable to be swept down by the floods and buried in the river gravels and alluvia, just as the squirrel is absent from both, because of its arboreal habits: it has left the gnawed nut-shells in the Cromer forest bed as

* The term Pleistocene is used in this essay as the precise equivalent of the term Quaternary, and as the natural name of the stage succeeding the Pleiocene. The Forest-bed mammalia, which I have formerly included in the Preglacial division of the Pleistocene, are relegated to the passage beds of the Pleiocene, or the Upper Pleiocene. Evidence for this change of nomenclature will shortly be published.

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Physiography of N. W. Europe in Pleistocene Period.

the only proof that it lived in Britain at that remote time. An examination also of the tables of the distribution of the animals proves, that so far as Great Britain and Ireland are concerned, the attempt to form a chronology, based on the presence of the mammoth, reindeer, cave-bear, and bison, has failed, because all the species are found indifferently mixed up together under the same conditions. I shall therefore treat the mammalia, both from caves and river beds, as being of the same age and belonging to the same fauna.

The Pleistocene land fauna of Great Britain, consisting of forty-eight species, may be divided into five well-marked groups, which will be examined separately: the first comprehending all the extinct species; the second, those still inhabiting the temperate zone of Europe; the third, those common to northern and tropical climates; the fourth, those confined to southern climates at the present day; and lastly, those confined to the inclement regions of the north.

The extinct group consists of nine, or perhaps ten, animals: the sabre-toothed machairodus, of Kent's Hole, and the narrow-toothed *Elephas antiquus*; the two slenderly-built rhinoceroses, the megarhine, and the R. hemitœchus of Dr. Falconer (*R. leptorhinus* of Owen), the tichorhine species, the cave-bear, the Irish Elk, the *Cervus Browni*, the mammoth, and possibly the *Hippopotamus major*. The last may possibly be represented at the present day by the larger African species. The first four and the last of these animals were of a southern habitat in Europe: the cave-bear, the Irish elk, and (*Cervus Browni*) Brown's deer—the representative of the fallow deer of the Mediterranean—were dwellers in the temperate zone; while the mammoth was almost cosmopolitan, occurring alike in France and Spain, and living alike on the fruit and leaves of the Scotch fir in Siberia, and the rich vegetation of the lower basin of the Mississippi. The woolly rhinoceros, on the other hand, was an animal that did not range in the Pleistocene times further south than the Alps and Pyrenees, while northwards and eastwards it extended as far over the whole of Siberia, and the now submerged area to its north, near the mouths of the Lena and the Indigirka. The Irish elk is the only animal that survived its extinct companions of the Pleistocene age, ultimately to disappear from the face of the earth during the time that the pre-historic peat-bogs and alluvia were being accumulated.

The mammoth and the woolly rhinoceros are the only two extinct forms that characterise the Pleistocene deposits, being found neither in the pleiocene nor in the pre-historic strata.

The second group, consisting of those Pleistocene species which still inhabit the temperate zones of Europe and America,

twenty-seven in number, links together the ancient with the present fauna. The bison, or the aurochs, ranged over nearly the whole of the Euro-Asiatic continent in Pleistocene times, from the Pyrenees through France and Germany and Siberia, as far as Behring's Straits and Eschscholtz Bay, on the American shore of the Arctic Sea, in company with the mammoth and the reindeer. In the days of Charlemagne it inhabited the forests in the neighbourhood of Aix-la-Chapelle, and some thirty years ago it was met with in the Caucasus, and at the present day it lingers in the Lithuanian forests, protected by a special decree of the Czar of Russia. The American variety inhabits the temperate zone of North America. I agree with Professor Brandt, of St. Petersburg, in viewing the American bison as a variety of the European, after a careful comparison of the points of difference between the two animals. The Pleistocene bison stands half way between the European and the American, and represents sometimes the characters of the one and sometimes the characters of the other. The Urus is still living, as Professor Rutimeyer tells us, in our domestic breeds, and it inhabited the woods of Germany in a feral state at least as late as the days of Charlemagne. The grizzly bear, for the identification of which we have to thank Professor Busk, is to be met with only in the temperate regions of North America. During the Pleistocene age it was more numerous in Britain than any of the other species of bear. The rest of the group may be passed over without further notice. They consist of

| | | | |
|---|---|---|---|
| The greater horseshoe bat | Fox | Shrew | Field mouse |
| | Ermine | Stag | Meadow vole |
| Common bat | Stoat | Roe-deer | Hare |
| Mole | Weasel | Wild boar | Rabbit |
| Wild cat | Martin cat | Horse | Common mouse. |
| Lynx | Otter | Beaver | |
| Wolf | Browbeat | Water rat | |

The Celtic short-horn (*Bos longifrons*) and the goat (*Capra hircus*) have been purposely omitted, because there is no evidence that either inhabited Great Britain or any part of Europe during Pleistocene times.

The third group, consisting of species common to cold and tropical climates, is represented only by the panther, which has been discovered by Mr. Sanford and myself in the caves of the Mendip Hills. On the continent it is known under the name of *Felis antiqua*. It is by no means rare in the caves. At the present day it is found throughout Africa, Asia Minor, Palestine, and the Altai mountains in Siberia.

We come now to the species found at present only in southern climates, the cave lion and the cave hyæna. The first of these is identical with the lion of Africa and Asia, and lived in the mountainous districts of Macedonia at the time that Aristotle was writing his "Natural History." The second I am unable to distinguish from the spotted hyæna now found only in South Africa. Both these animals were, on the whole, larger than their living representatives, a difference which probably was caused by the abundance of food which they obtained, the hunters in those early days not being sufficiently numerous to compete with the carnivores for their prey.

The mammalia, composing the fifth and most important group, now confined to the colder regions of the north, or to high altitudes in the northern hemisphere, consists of the glutton, reindeer, musk sheep, pouched marmot, tailless hare, and the lemming.

From this short analysis of the Pleistocene fauna, we can realise the peculiar mixture of extinct animals with those now vanished to different regions of the earth. We have now to examine the conditions under which they must have lived in Europe during the Pleistocene age. We will take that relating to the climate first. The hyæna and the lion, living only at the present day in hot regions, imply at first sight that the temperature under which they lived in Europe was comparatively high. But, on the other hand, the fact that the tiger of the Altai is specifically identical with that inhabiting the jungles of Bengal, shows that the testimony offered by the carnivores as to climate is not altogether trustworthy. In the case of the lion, the temperature of the mountains of Thrace was undoubtedly infinitely more severe than that of any region in which it now lives, and most probably approaches to that under which it occupied the forests of Germany, France, and Britain. The spotted hyæna may very likely have been endowed with the same elasticity of constitution as the living tiger, which enabled it to live in far colder regions than that in which it is now found. We may therefore dismiss the evidence of both these animals as to climate, as being liable to lead into error. The presence, however, of the genus hippopotamus in the Pleistocene fauna cannot be accounted for except by the hypothesis that during the time it lived here the climate was temperate, or even warmer than it is now. All the species of hippopotamus now alive spend the greater part of their lives in the water, rather than on the land. It is therefore extremely improbable that any ancient species, so little removed in form from the living, could have inhabited a country in which the rivers were frozen for a considerable portion of the year. Had the Pleistocene hippopotamus been

endowed with habits and modes of life different from those of the living species, it is scarcely possible that this difference should not have impressed itself on the skeletons which attest its presence in Europe. And yet the difference is so small between the extinct European and the existing larger hippopotamus, that it is scarcely appreciable. The animal, therefore, indicates that during the time it occupied Europe, the rivers were free from ice..

This view is, however, directly opposed by the evidence of the whole group of Arctic mammalia, which lived in central and northern Europe during the Pleistocene age. It is incredible that the climate suited for the well-being of the hippopotamus could at the same time have been adapted for the reindeer, the lemming, or the musk sheep; for we have no reason to believe that the powers of resisting heat or cold possessed by these animals ever differed from those which they now possess. The evidence, therefore, afforded on the one hand of a warm Pleistocene climate is balanced on the other by that in favour of its having been as severe as that under which the northern group of mammalia now flourish. And the opinions of eminent naturalists are, as near as possible, equally divided as to its actual character. M. Lartet, fixing his attention more particularly on the hippopotamus, inclines to the former view, while the latter is that taken by Dr. Falconer and Mr. Prestwich. The two views are, however, by no means antagonistic, if we suppose that in Pleistocene Europe the climate was somewhat similar to that of the vast plains of Siberia, extending from the Altai mountains to the Arctic Sea, or to that offered by the inland climate of North America. In Siberia we meet with every gradation in climate, from the temperate down to that in which the cold is too severe to allow of the growth of trees, which gradually decrease in size as the traveller passes northwards, and are replaced by the grey mosses and lichens of the low, marshy tundras. Throughout the north the winter cold is intense, and in the southern portion is almost compensated for by the great summer heat, and its marvellous effect on vegetation. In the north countless herds of reindeer and elks, followed by wolves, foxes, bears, and gluttons, are continually on the move, in the heats of summer passing northwards, and avoiding the severity of the winter by withdrawing for shelter into the forests in the south. If the reindeer retreat far south, a severe winter is to be apprehended; if they remain very nearly in their usual haunts, the season is invariably a mild one. There is, indeed, a continual swinging to and fro of the Siberian mammalia; the reindeer sometimes invading the province occupied by the elks and reddeer, while at others the latter animals encroach upon the province

of the reindeer. . The North American mammalia also vary in their range according to the climate, as Sir John Franklin found, to his cost, when he was travelling over the barren grounds from the shore of the Arctic Sea. In both America and Siberia there is a zone of debateable ground, in which the mammals of the Arctic and temperate provinces are continually oscillating to and fro, according to the seasons. And in this their skeletons could not fail to be mixed together in the deposits of the rivers. The musk sheep, for instance, which in Herne's day, A.D. 1772, lived near Fort Churchill, has now left that district to be occupied by the elk and the wapiti.

There can be no doubt that the mixed character of the Pleistocene fauna in Britain and Central Europe is due to a similar oscillation to and fro of the animals according to the seasons; and when we consider the geographical position of that area at the time (Pl. LXXVIII.), we can see at once how the mammalia occupying it must necessarily have been mixed. The land stretched continuously without any impassible barrier northwards and eastwards to the present home of the reindeer in Euro-Asia; while, on the other hand, it reached southwards over a considerable portion of what is now the Mediterranean, almost, if not quite, touching Africa. The winter cold and the summer heat of so great a mass of land must necessarily have been more severe than now, when the Mediterranean occupies a far wider area, and when the Atlantic and the Baltic and the North Sea have considerably diminished the area of the land. It is therefore by no means to be wondered at that a southern animal, such as the hippopotamus, should have wandered northwards and westwards as far as the latitude of Yorkshire, and it is worthy of note that this is the extreme northern limit of the range of the animal. On the other hand, during the severity of winter the reindeer and the musk sheep descended southwards, and occupied the area which they deserted at the approach of summer. Such, in my belief, is the explanation of the mixed character of the Pleistocene fauna; it arises partly from the climatal extremes which must result from the extension of the European continent over what is now sea. The continuity of land also northwards and southwards afforded room for the swinging to and fro of the northern and southern forms of life. When that continuity was broken the animals would be cut off from their bases of retreat, and disappear from a region in which the climate was passing from a continental to an insular condition. It must, however, be admitted that the enormous preponderance of northern over southern animals in Pleistocene Europe implies a great severity of winter cold, while the comparatively few remains of southern animals show that they were rarely able

to invade the country of the reindeer. All the remains of fossil hippopotamus in this country which I have seen, with two exceptions, belong to adults, and it is very probable that that animal seldom or never bred in our country. The mammoth has purposely been omitted in this analysis of the evidence afforded by the mammalia as to the climate, because it happens to be one of the few creatures which were able to live under very different climatal conditions, being found alike in the volcanic ash in which Rome is built, the frozen marshes of Siberia, and in the morasses of the Southern States.

Nor does this evidence as to the Pleistocene climate stand alone. The contorted gravels, and the angular state of the pebbles of which they are often composed, are, as Mr. Prestwich infers, explicable only on the theory of ice having been formed in our rivers in larger quantities than at the present day: the one being the result of the grounding of large masses of ice, and the other of their melting away, and consequently dropping their burden of pebbles. The large plateaux of brick earths are also probably deposited by floods, caused, like those of Siberia and North America, by the sudden melting of the winter snow.

This consideration of a Pleistocene climate leads necessarily to the difficult problem of the relation of the Pleistocene mammalia to the period of intense cold, the Glacial period; and before this can be discussed, I must define exactly what I mean by the term. At the close of the Pleiocene period the temperature of northern and central Europe became lowered to such a degree that it became almost Arctic in character, and those complex phenomena were manifested which we know as glacial. And the latter indicate geographical changes of enormous magnitude. The researches of many eminent observers prove, that at the commencement of the Glacial period an enormous sheet of ice, like that under which Greenland now lies buried, extended from the hills of Scandinavia over North Germany, the North Sea, Scotland, Ireland, Cumbria, and the hilly districts of England, at least as far south as the valley of the Thames. The land then, most probably, as Professor Ramsay and Sir Charles Lyell believe, stood higher than it does now. Then to this succeeded a period of depression, during which the mountains of Wales were submerged to a height of at least 1,300 ft.; and the waves of the sea washed out of the pre-existing glacier detritus the shingle and sand, termed the 'middle drift,' of the North of England and of Scotland and Ireland.* Then the land was re-elevated above the

* I have to acknowledge the kind assistance of Professor Hull, F.R.S., Mr. Kinahan, and the Rev. M. H. Close, in this portion of the subject.

waves, and a second period of glaciers set in, traces of which occur abundantly in Wales, Scotland, Ireland, and even as far south as Dauphiné and Auvergne. They were, however, of far less extent than those which preceded them, occupying isolated areas instead of forming one continuous icy covering to the country. Such as this is a brief *résumé* of the glacial phenomena: 1. As the pleiocene temperature was lowered, the glaciers crept down from the tops of the mountains, until at last they formed one continuous ice sheet, moving resistlessly over the smaller hills and valleys to the lower grounds, and the first glacier period set in. 2. Then followed the period of depression beneath the sea. 3. And, lastly, on the land re-emerging from the sea, the second glacier period set in. The climate during the marine depression must obviously have been milder than that of either of the glacier periods, because of the moderating effect of the presence of a stretch of sea.

What is the precise relation of the Pleistocene mammals to these two glacier periods? Did they invade northern and central Europe during the first or the second, before or after, the marine submergence indicated by the "middle drift?" We might expect, *à priori*, that as the temperature became lowered the northern mammalia would gradually invade the region occupied before by the pleiocene forms, and that the reindeer and the mammoth would gradually supplant the *Cervus ardens* and the *Elephas meridionalis*. Traces of such an occupation would necessarily be very rare, since they would be exposed to the grinding action both of the advancing glacial sheet, and also subsequently to that of the waves on the littoral zone during the depression and re-elevation of the land. At the time also that the greater part of Great Britain was buried under an ice sheet, they could not have occupied that region, although they may have been, and most probably were, living in the districts further to the south, which were not covered by ice. The labours, however, of Dr. Bryce and others proved that one at least of the characteristic Pleistocene mammalia—the mammoth as well as the reindeer—lived in Scotland before the deposit of the lower boulder-clay; while Mr. Jamieson has pointed out that they could not have occupied that area at the same time as the ice, and therefore must be referred to a still earlier date.* The teeth and bones discovered in the ancient land surface at Selsea also very probably indicate that the mammoth lived in Sussex

* For account of these discoveries, see "Trans. Geol. Soc. Glasgow," vol. i. part 2; "Quart. Geol. Journ.," vol. xxi. pp. 161 *et seq.* and pp. 204 *et seq.* I am also indebted to Mr. James Geikie for valuable information on the subject.

before the glacial submergence, although they were never admitted by Dr. Falconer to be of the same age as the remains of *Elephas antiquus* from the same preglacial horizon. On a careful examination of the whole evidence, I am compelled to believe, with Mr. Godwin-Austen and Mr. Prestwich, that the *à priori* argument that Pleistocene mammalia occupied Great Britain before the first glacier period to be fully borne out by the few incontestable proofs that have been brought forward of the remains being found in preglacial deposits. And the scanty evidence on the point is just what might be expected from the rare accidents under which the bones in superficial deposits could have withstood the grinding of the ice sheet and the subsequent erosive action of the waves on the coastline. This view seems to me to be more likely to be true than that which I have hitherto maintained, that the Pleistocene mammalia arrived here after the marine submergence, and to which I had been led partly by the doubts of Dr. Falconer as to the age of the mammoth at Selsea, and partly from my own doubts whether the clays under and on which the animal was found in Scotland belonged to the first or to the second period of glacier extension; while, on the other hand, the postglacial range of the Pleistocene mammalia in central and eastern England was clearly proved in many cases.

But whatever view may be held as to the arrival of the Pleistocene mammalia in Britain during the lowering of the temperature which immediately preceded the first glacier period, an examination of the accompanying map (Pl. LXXVIII.) will prove that they were in full occupation of the low country at the time that the higher lands and certain other regions were occupied by the ice during the second glacier period. The dotted areas are those in which the Pleistocene mammalia have been found in Scotland, Ireland, Wales, and England, Northern France and Belgium, and which occur equally in the bed of the North Sea and in the British Channel; while those areas which are left plain on the map are those which are full of the most fresh-looking traces of ice action, old moraines, glacial striæ, and the like, which have a direct relation to the existing valleys.

The absence of these animals from those areas must have been caused by the existence of some barrier to their migration; and the hypothesis that this was the presence of ice alone satisfies all the conditions of the case. It accounts both for the exceedingly modern aspect of the glacial phenomena, and for the irregular distribution of the animals.* We may

* The authorities for the distribution of the mammalia are to be found in my Essay published in "Quart. Geol. Journ.," May 1869.

therefore be tolerably certain that the Pleistocene mammalia lived here at the same time that the glaciers still covered large areas in Great Britain and Ireland.

The consideration of the Pleistocene climate is also intimately connected with the former extension of North-Western Europe into the Atlantic; for the extremes of temperature implied by the mixed character of the former can only be satisfied by the view that Great Britain was not an island, but an integral portion of a continent. In the following map, which is based on that drawn by Dr. Petermann and published by Dr. Stieler, I have followed Sir H. de la Beche and Sir Charles Lyell in taking the 100-fathom line as representing the coast, the time from which the soundings deepen seawards so quickly in every direction, that the line of 200 fathoms would include an area which is but slightly larger. As evidence of this coast line, Mr. Godwin-Austen has brought forward the littoral shells, the shingle, and the line of rocks which are found near the embouchment of what may be called the river of the English Channel. And that this river is no myth is proved by the discovery of the *Unio pictorum*, in from 50 to 100 fathoms water, by Captain White, at the same point.

To complete this very brief sketch of the physiography of Pleistocene Britain, I have inserted the rivers, and have traced them by the soundings to their mouths in the Pleistocene sea. A glance at the map will show the relation of the present rivers to those great arteries to which they once contributed their waters. It is obvious that the great valleys of the English Channel and the North Sea, and probably that of the British Channel, would afford free scope for the migration of the Pleistocene mammalia from France and Germany to our country, and that of the Irish Channel to Ireland. And it is by no means remarkable that the paleolithic savages who lived on the banks of the Somme or the Seine, or in the caves of Belgium, should have left like traces of their presence in the south and the east of England. Had they merely occupied one side only of what must have been to them a most valuable hunting-ground, and not sometimes have crossed over to the other, we should have cause to wonder at their caprice.

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## EXPLANATION OF PLATE LXXVIII.

**Dotted Areas** = those in which Pleistocene mammalia have been found.
**Lined** „ = the extension of land to the 100-fathom line.
**Plain** „ = Glaciers. Figures = fathoms.

# STAR STREAMS AND STAR SPRAYS.

By RICHARD A. PROCTOR, B.A., F.R.A.S., Author of "Other Worlds," "The Sun," "Light Science," &c. &c.

THE stellar heavens present us with a problem of vast difficulty—the problem of determining the laws according to which those myriads of orbs which the unaided eyes can see, or which the telescope reveals, are distributed throughout space. We can determine the laws of stellar distribution so far as they relate to the imaginary concave of the heavens. We could form a globe upon which millions of stars might be indicated; or, better, we might mark these millions of stars upon the interior surface of a hollow globe; and thus, so far as the apparent laws of stellar arrangement are concerned, we might actually render the eye cognisant of all which even the most powerful telescopes can reveal. But when this had been accomplished, we should have made but a short step towards the determination of the manner according to which the stars are distributed throughout space. We should have placed all the stars which the telescope reveals upon a spherical surface, whereas we know that they lie in reality on no such surface, but some at distances much vaster than the distances which separate us from others. We know that if we have to deal with a sphere of stars at all, it is a sphere *full of* stars, and not a spherical surface *covered with* stars, that we have to consider. But, in truth, we know that the space containing all the stars revealed by powerful telescopes may not even approach the form of a sphere; and that within that space the stars may be distributed in the most irregular manner—here crowded most densely, here sparsely scattered, and throughout enormous regions mayhap altogether wanting; in some places arranged into clustering aggregations, in others in streams, and elsewhere in fantastic convolutions or reticulations; while, for aught that has yet been shown, the whole stellar region may be occupied more or less richly with a variety of forms of matter other than stars or suns, and even differing perhaps from any forms of matter with which we are acquainted.

Unfortunately we have no means of modifying the conditions of the problem presented to us by the stars. We must be content to analyse patiently the evidence by means of which alone the problem can be attacked. Whether when that has been done, or while that is being done, we shall be able to form clear and definite conclusions respecting the stellar system, need not at present concern us. It is at any rate certain that if the secret of the heavens is to be disclosed, it can only be as the result of systematic inquiries directed to the vast mass of information which has already been gathered together; and the doubts we may entertain as to the actual fruits of such inquiries ought not to deter us from making them, since that would be in effect to give up the problem of the star-depths altogether. To use the words of the astronomer to whom more than to any other, save one alone, we owe the power itself of making such inquiries, we must "not be deterred from dwelling consecutively and closely on speculative views by any idea of their hopelessness which the objectors against 'paper astronomy' may entertain, or by the real slenderness of the material threads out of which any connected theory of the universe (at present) has to be formed. *Hypotheses fingo* in this stage of our knowledge is quite as good a motto as Newton's *non fingo*, provided always they be not hypotheses as to modes of physical *action* for which experience gives no warrant."*

My purpose in the present paper is to pursue an inquiry (commenced by me some five years ago) into a certain peculiarity of the arrangement of objects within the star-depths, which appears to promise some insight into the real laws of stellar aggregation. I refer to the circumstance that there may be observed among the stars a tendency to arrangement in streams, of greater or less length, and more or less distinctly recognisable. I offer at present no explanation of the observed fact, but seek rather to convince the reader that this peculiarity has a real existence, and that it may be regarded as in fact a characteristic peculiarity of the stellar system. It must be mentioned, however, that the tendency to stream-formation among the stars is not to be regarded as universal. On the contrary, it is but a sign of a much more general law, according to which the stars are found to aggregate in certain regions and to be segregated from others, as though, in some long past era, forces had been at work which drew the star material towards certain regions of space, to the avoidance of others. And here again, I would invite attention to the fact that the study of these laws of stellar aggregation and segregation seems to afford the only means of attacking a problem of immense

* From a letter to the present writer, August, 1869.

difficulty, to which the inquiring mind is naturally led by the partial success which has attended inquiries into the origin of our own solar system. We recognise so clearly within our solar system such motions and such laws of distribution as suggest a process of evolution, that the mind is led to inquire whether the motion of the stars and their arrangement throughout space may not indicate the action of a yet higher order of evolution. If the genesis of a solar system has been or is being revealed to us, may not the genesis of a galaxy be one day revealed in like manner?

But I merely point to such inquiries as these, in passing, by way of indicating the class of questions to which such phenomena as I am about to consider may eventually lead. Let us now turn simply to the discussion of those observed facts which seem to show that the stars in certain regions have been gathered into streams.

If we consider the stars according to their various orders of apparent magnitude, we are, in fact, treating the problem precisely as though the celestial vault were studied by means of telescopes progressively increasing in power. Nor need we, in so doing, make any assumption as to the real magnitudes of the stars. We know quite certainly that whatever telescopic power we use, or even when we study the heavens with the naked eye, we have to deal with objects lying at different distances, and that, therefore, we are exposed to the possibility of error arising from the fact that orbs which seem to be associated may in reality be in no way connected. But if we keep this possibility of error very carefully in view, and if we apply to the various cases which come under our notice such laws of reasoning as may best serve to eliminate such error, then, although we may not be sure that in all instances the error in question has been obviated, we shall yet have done much to obtain at least probable evidence respecting the laws of stellar distribution.

As an illustration of my meaning, I will take an instance belonging to the more general law of stellar arrangement, of which stream-formation is but an instance. The reader is aware that the six stars which ordinary powers of sight recognise in the Pleiades, are but a few among a very large number which are seemingly collected towards one particular region of the heavens in this place. Now, if we consider *only* two stars of the Pleiades, considerably unequal in magnitude, it must be regarded as not only possible, but (on *à priori* considerations) highly probable, that these two orbs lie at very different distances from the earth, and are not physically associated. But we are not free to extend this reasoning, which is admissible in the case of two stars, to the whole group of the Pleiades, and to

argue that, because we have no means whatever of determining the actual distances of the orbs in that group, we are not at liberty to assume that they form a real clustering aggregation of stars. In so doing, we should undoubtedly be losing sight of evidence which absolutely demonstrates the clustering nature of the Pleiades. We have only to consider the mathematical probability that so many orbs would be gathered together within a certain portion of the heavens in the Pleiades, when the total number of stars between the same limits of magnitude is such and such, to see that we have not to do with an accidental phenomenon due merely to the apparent association of stars of many orders of distance in nearly the same direction, but with a real aggregation of stars into a definite cluster, surrounded on all sides by comparatively vacant regions. We know that William Mitchell, more than a hundred years ago, by simply considering the six brighter stars of the Pleiades, was able to show that the odds are about half a million to one against the association of these stars being apparent only.*

Now it is worthy of notice that, even among stars of the first three or four orders of magnitude, signs of aggregation are discernible, which appear too marked to be due to mere chance distribution. For instance, if we take an equal-surface (*isographic*) chart of the northern heavens, showing all stars down to the fourth magnitude inclusive, we are struck by the singular vacancy lying where modern astronomers place the constellation of the Cameleopard. Within an oval space, having Polaris and Castor as the ends of its longer diameter, Dubhe and $\delta$ Aurigæ as the ends of its shorter diameter, there are but three stars (of the fourth magnitude), although this region extends

* Mitchell's paper, in Vol. lvii. of the *Philosophical Transactions*, anticipates in the clearest possible manner one of the general laws of stellar distribution which I have lately endeavoured to establish. The following passage, in particular, may be quoted in illustration:—"It has always been usual with astronomers to dispose the fixed stars into constellations; this has been done for the sake of remembering and distinguishing them, and therefore it has in general been done merely arbitrarily and with this view only; nature herself, however, seems to have distinguished the stars into groups. What I mean is, that from the apparent situation of the stars in the heavens, there is the highest probability that, either by the original act of the Creator, or in consequence of some general law (such, perhaps, as gravity), they are collected together in great numbers in some parts of space, whilst in others there are either few or none. The argument I make use of in order to prove this is of that kind which infers either design or some general law, from a general analogy, and the greatness of the odds against things having been in the present situation, if it was not owing to some such cause."

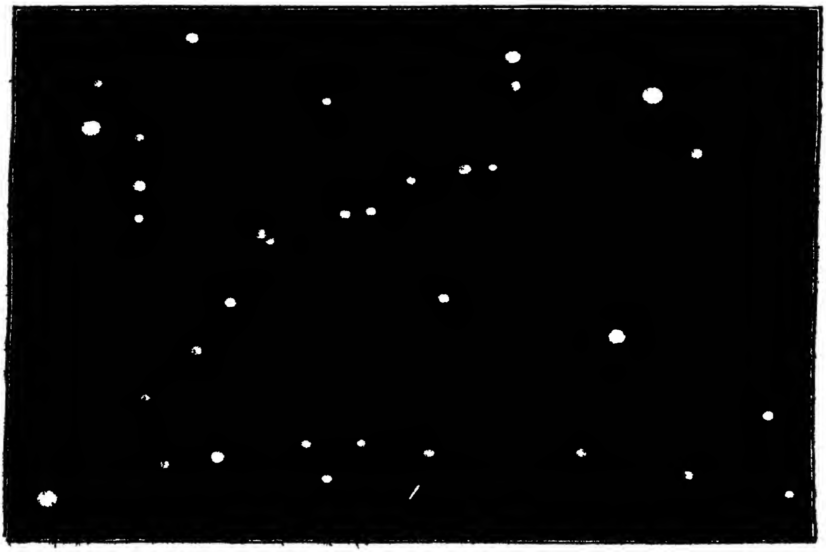
over some fifty-eight degrees in length and about thirteen degrees in breadth.

But it is when we consider the stars down to the fifth magnitude inclusive that we first begin to recognise the existence of a marked tendency to stream-formation. It is among these stars, in fact, that we find those streams which the ancients recognised when they gave to certain star-groupings such names as Hydra, Draco, Serpens, the River Eridamus, and when they marked down among the constellation-pictures two streams from the water-can of Aquarius and a band connecting together the two fishes. The prolongations of some of these streams of lucid stars have been recognised by those modern astronomers who gave to certain southern star-groupings the names Hydrus, Reticulum, and the like.

Now, the chief question which has to be answered, in considering the evidences of stream-formation, is whether the streams are apparent only or real; and, in order to answer this question, we have to inquire what form or degree of streaminess (so to speak) might be expected among the 1,500 stars, down to the fifth magnitude inclusive, if these were really spread at random over the celestial sphere. In the "Popular Science Review" for July 1870, I have indicated the means whereby I have tested this matter, and the conclusion to which I have been led—*this* namely, that although among 1,500 or 2,000 points distributed at random over a surface of any kind, certain groups resembling streams might be recognised, such streams would not be nearly so well marked as the streams actually observed among the stars down to the fifth magnitude. But, on the other hand, it is not to be expected that the star streams actually recognised should be so exceedingly well marked and regular, or should be traceable over such great distances, that the reality of the stream-formation must be obvious at once. Had this been the case, indeed, the reasoning by which I have endeavoured to establish the reality of the phenomenon would not have been required. The first astronomers would have recognised the phenomenon as clearly as we can do. Therefore I do not consider the arguments which have been chiefly urged against these streams of lucid stars, regarded as having a real existence, as needing refutation. It has been urged that the streams can only be traced over such and such distances; that they can be carried this way or that, according to fancy, and so on. This, however, was to be expected; if it were otherwise, the reality of the streams would long since have been recognised; and apart from this, remembering that we are looking into the depths of space, and that, supposing star streams really to exist, we must see them foreshortened—in many instances projected on a background of

stars less systematically distributed, and in other cases *mixed up* seemingly with other streams, either nearer or further off—the wonder rather is that any well-marked *portion* of any stream should be recognisable, than that no stream should be traceable over very large areas on the heavens, and still less from its beginning to its end. That the reader may form his own opinion as to the reality of the streams traceable among stars down to the fifth magnitude, I give the case of a star-group which is certainly not the most remarkable for streaminess, but chances to be more convenient for the purposes of illustration than most others. Fig. 1 presents the stars forming the

Fig. 1.



Showing the stars which form the connecting band of Pisces, &c

connecting band of Pisces. The bright star in the lower left-hand corner is the knot of the band, one part of the band being formed by the curved stream of stars passing to the lower right-hand corner, the other by the curved stream passing, with an inflection near the double star in the figure, towards the upper right-hand corner. In this figure the fact that certain sets of stars lie on certain curved lines is of slight significance, for assuredly in any chance distribution of stars the like would be found; the fact which is really significant is the paucity of stars on either side of the curved streams. We have certain lines along which the stars are plentifully strewn, while the adjacent spaces are relatively vacant. This feature, recognisable not only in this case, but in others, and even more

markedly in several instances, is one which cannot reasonably be ascribed to mere coincidence. Let it be noted, moreover, that whatever significance we attach to it, when considering the stars of the first five orders of magnitude, must be enhanced if, as we proceed, we recognise a similar feature (on a different scale, however) among stars of lower orders of magnitude. Throughout this paper, I am not presenting a series of considerations so related one with the other that the failure of one destroys the validity of my reasoning; I am dealing with arguments which are independent of each other, though severally adding to each other's strength. If some of them fail, my case is only *pro tanto* weakened; it is not by any means destroyed.

Before leaving Fig. 1, however, I would invite special attention to the manner in which the two star streams are conjoined. We see these streams converging upon a single star brighter than those which form the streams themselves; and we may also trace, not indistinctly, a certain general equality of distribution among the stars of the two streams. The former feature is, however, the only one I care at present to dwell upon; and it is to this particular arrangement of streams—two or more (but usually two) proceeding from a single star—or of branches proceeding, as it were, from a single stem, that I have given the title of star sprays. In searching among the star-depths revealed by telescopes of considerable power, many cases may be noticed in which such star sprays exhibit a singular uniformity of structure. The stars of the leading magnitudes are too few in number to afford many well-marked instances. I may note, however, the arrangement of the stars in Coma Berenices as one illustration of this sort; the stars $\gamma$, 14 and 13, forming the stalk of the spray. Another illustration may be recognised in the stars forming the poop of Argo and the hind-quarters of Canis Major, or (to use a more satisfactory way of indicating the orbs I refer to) the streams of stars converging on $\xi$ and $\rho$ Argûs, from $\epsilon$ Canis Major and from $\pi$ Argûs. At $\epsilon$ Canis Major there is another subdivision; one stream of stars passing to $\kappa$ Columbæ, the other over $u$ and $x$ Puppis to $\nu$ Argûs. The streams from the water-can of Aquarius form a more extensive, but perhaps less satisfactory, illustration of the same peculiarity.

I need give the less attention to those cases of stream-formation which may be recognised among the stars of the first six orders inclusive, because I have already discussed the relations among the stars, in the second edition of my "Other Worlds." Of the peculiarities of *distribution* recognisable among the stars there dealt with, I may say with confidence that it is wholly impossible to regard them as accidental; they indicate

beyond all possibility of question the existence of some real cause which has led to a drifting of the stars towards certain regions. As regards such peculiarities of arrangement as would fall more particularly under the head of my present subject, I think it is almost equally impossible to feel any doubt. If some of the streams and reticulations which can be recognised in the isographic chart added to the second edition, be due to chance distribution, the coincidence is very much more remarkable than the theory of star streams which I am at present advocating. It is truer to say, however, that the laws of probability as at present understood will not permit us to regard such singular configurations as accidental.

It would be desirable that we should have equal-surface charts of the heavens to include stars down to the seventh, eighth, and ninth magnitude severally; because it is only by thus considering the separate stages of space-penetration that we can obtain complete recognition of the laws of stellar distribution throughout space. We owe, I think, to the elder Struve the first recognition of the importance of such graduated advances within the star-depths; though he dwelt rather on the importance of star-gauging (and that, also, according to averages) than on the value of star-charts capable of revealing to the eye the statistics of stellar distribution. It will not be difficult to construct charts including stars down to the seventh, and eighth, and ninth orders of magnitude; because as soon as the complete survey of the heavens has been effected after the plan already extended by Argelander to the northern hemisphere, the charts forming the survey, if carefully drawn,* will enable us to construct charts of complete hemispheres including stars down to the seventh, eighth, and ninth magnitudes severally inclusive.

At present, however, for want of such intermediate charts (so to speak) I pass from my equal-surface projection of all the stars down to the sixth magnitude inclusive, to an equal-surface projection which I have just completed, in which all stars in Argelander's series of forty northern maps have been marked in with careful reference as well to their arrangement as to their magnitude. In these forty charts, as many of my readers

* It is important that the size of the discs used to indicate the several magnitudes should remain unchanged during the whole process of engraving, and also that the several charts forming the series should be printed with exactly the same degree of *fulness*. In Argelander's splendid series of forty charts, in which all the northern stars down to the magnitude intermediate between the ninth and tenth are included, slight changes have taken place during the progress of the work, which creates some degree of doubt as to the orders to which the stars belong in some of the charts.

are doubtless aware, Argelander has included all stars down to magnitude nine and a-half, within ninety-two degrees of north polar distance—the two degrees south of the equator being added in order to facilitate the comparison of the northern atlas with charts forming the southern survey, one day to be completed (it may be hoped) at southern stations. In all there are 324,198 stars. All these I have carefully copied in, upon a circular chart two feet in diameter, isographically divided (in pencil) by radial lines and circles, into spaces extending one degree in declination and one degree in right ascension for sixty degrees to the north of the equator, the nominal extension in R.A. being correspondingly increased with proximity to the pole. In fact, all the spaces in Argelander's series of charts (some 26,400 in all) were represented in pencil in my projection, before a single star was charted in. Then the stars were carefully copied in, space by space, from Argelander's atlas; at such a rate (on the average), that the whole work of charting occupied me about 400 hours.* I do not think that the labour was thrown away, when it is remembered that, as a result, the statistical distribution of all the stars down to the 9½th magnitude was presented to the eye. The gauges of the Herschels had included in all about 160,000 stars, and Struve, in the elaborate series of inquiries on which he founded the theories propounded in his 'Études d'Astronomie stellaire,' dealt with about 32,000 stars; but the labours of Argelander enabled me not merely to count, but to delineate, 324,198 stars —not merely to draw inferences from statistical enumeration as to the real laws of stellar distribution, but to exhibit those laws of distribution to the eye.

Now the first and most important conclusion deduced from this process of charting relates to the Milky Way; and it will be well to defer the consideration of that conclusion until I come to speak of the Milky Way as itself a vast conglomeration of star streams and star sprays.

But another conclusion, not obviously deducible from the chart itself, or its photographic reductions, was forced upon me in a very marked manner as the work of charting proceeded. Again and again I had occasion to notice the tendency of the

* Argelander and his assistants were engaged no less than seven years and one month in completing their magnificent contribution to uranography. The rate at which I copied in the stars was such as to enable me to copy carefully within each space on my projection the stars shown in the corresponding space in the large atlas. It is worthy of notice that a single second of extra time (on the average) per star would have caused an addition of ninety hours', or say ten days', work. The time actually employed on the average was slightly less than four and a half seconds per star.

stars to associate themselves into streams and sprays, the star sprays being ordinarily of the form illustrated in Fig. 1. It would be quite impossible for me to convey by means of pictures any adequate idea of the persistence with which these peculiarities of structure were renewed, especially in certain parts of the chart. Indeed, it would be absolutely necessary to include a much larger portion of the heavens than could here be conveniently pictured, to show the most significant feature of all, the manner namely in which the sprays are divided and sub-divided.

Now the point to be chiefly noticed here is, that so far as *à priori* considerations are concerned, one would be led to expect that no very marked signs of stream-formation would be shown among stars down nearly to the tenth order of magnitude. For the further we increase our range of vision, the more likely must we be, it would seem, to obtain a view in which the actual relations of the stars are confused by seeming combinations, due to the accidental agreement, in general direction, of star-groups at very different orders of distance. For either there is or there is not a general uniformity of star-magnitude (within certain limits). If there is such uniformity, so that the fainter stars are in reality as large (on the average) as others, but more distant, then it is certain that amongst the stars dealt with in Argelander's charts there are in all directions stars at very different distances; stars therefore not in reality associated, but which yet, being seen in nearly the same direction, are brought into seeming juxtaposition. If, on the other hand, there is not among the stars, even in a general sense, any approach to uniformity of magnitude, then we may be even more completely deceived; for certain near stars which are really very small (relatively) may be brought, not merely into seeming juxtaposition with more distant stars, but even to a seeming equality of magnitude.

This being remembered, it was to be expected that the distribution of the stars included in Argelander's charts would correspond much more closely to a real chance distribution of so many points over a hemisphere, than where we considered only a comparatively small number of stars belonging to the leading orders of magnitude. Moreover, even assuming the point which I am now endeavouring to prove, viz., that the stars are in many cases arranged into the form of streams and sprays, it would yet seem highly probable that all signs of these streams and sprays would be obliterated, simply because streams and sprays of stars would probably lie at very different distances in all directions, and the configuration of the nearer streams would be blended with and confuse the configuration of the more distant.

Figs. 2 and 3 will serve to illustrate my meaning. It is not difficult to recognise in both these figures the existence of star

Fig. 2.

A portion of one of Argelander's charts, the centre of the space here shown being nearly in 32½° N. Dec., and 10h. 28m. R.A.

streams and star sprays too well marked to be regarded as due to accident; but yet we are led to suspect that the streams

Fig. 3.

A portion of one of Argelander's charts, the centre being in about 26½° N. Dec., and 22h. 8m. R.A.

here seen lie at different distances, and that much more clearly marked streams would be recognised if we could cut off with a veil all the stars beyond a certain distance, and obliterate altogether certain of the nearer stars. I would, however, recommend such of my readers as possess Argelander's chart to study the region around the two spaces pictured in Figs. 2 and 3; when I think the conviction will be forced upon them that there is a much closer connection between the several branches of stars seen in those regions than one would have been disposed to expect among the orders of stars Argelander has included in his charts.

Many cases occur, however, in which two streams lying at different distances appear to cross each other in the chart; and it is a somewhat noteworthy circumstance, that the disposition of five nearly equal stars in the form of a cross, thus · ⁝ · which is very seldom met with (compared with other simple configurations) in the complete series of charts, is commonly to be noticed where two well-marked streams cross each other.

The arrangement of the stars in the large chart, as respects aggregation in certain regions and segregation from others, is sufficiently remarkable; but I have not space to dwell at length here on peculiarities of that description. Some of these peculiarities are associated with the configuration of the galactic stream of stars, presently to be briefly referred to. One, however, is so remarkable that I cannot refrain from here calling special attention to it. The Milky Way region or zone is shown in the chart to be exceptionally rich in stars (as W. Struve judged from statistical considerations); but instead of that gradual tendency to aggregation towards the galactic zone which Struve supposed to prevail, there is in many places a sudden change in the density of distribution, spaces close by the galaxy being relatively poor. But in no instance is this peculiarity so remarkably exemplified as in the part of the Milky Way near the horns of Taurus. Here we have on one side the rich fields of the Hyades and the Pleiades, and on the other rich galactic fields—properly so-called; but between these two rich regions we have absolutely the poorest region in the whole of the northern heavens.*

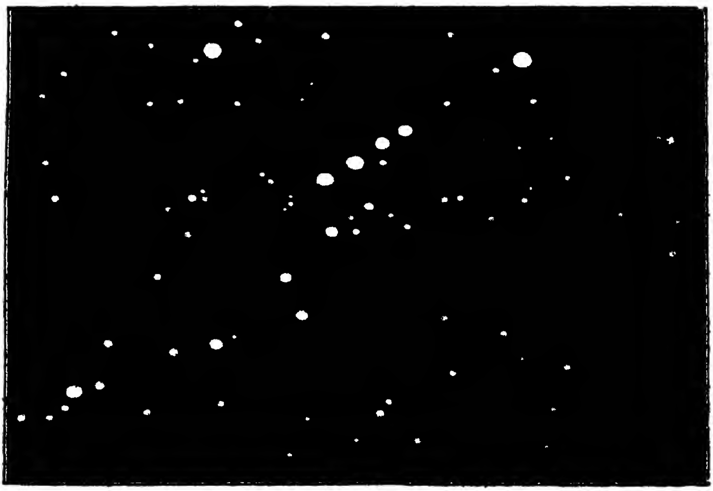
Extending next our range of view so as to reach the stars down to the thirteenth magnitude inclusive, we have indeed less complete surveys to consider, but yet the evidence we obtain is sufficiently distinct. The zodiacal zone has been closely surveyed by Chacornac, Hind, and others, with the

* This peculiarity did not escape the attention of Argelander, who says:—"Die absolut ärmste Gegend findet sich aber sonderbarweise nicht gar weit von der Milchstrasse entfernt, an den Hörnern des Stiers."

object of so mapping down even the fainter stars, that the asteroids which traverse this region may be the more readily recognised. In the maps thus constructed, we find star streams and star sprays as well marked as in Argelander's chart. As the same general considerations apply in this case, it will be sufficient for me to invite attention to Figs. 4 and 5; but I would recommend the student who may possess Chacornac's charts to study carefully the regions which surround the two spaces pictured in these figures.*

If we pass on towards yet more remote depths, we still find well-marked signs among the stars of a tendency to form

Fig. 4.



A portion of one of Chacornac's ecliptic charts, the centre of the space here shown being in 1° N. Dec., and 23h. 43m. R.A.

streams and sprays. Sir John Herschel has pictured some very singular specimens of such streams, as seen in his eighteen-inch reflector during his survey of the southern heavens; and doubtless, could the fields surveyed by the elder and younger Herschel be presented in maps, so that several adjacent fields could be seen at a single view, many other instances would be added to the list.

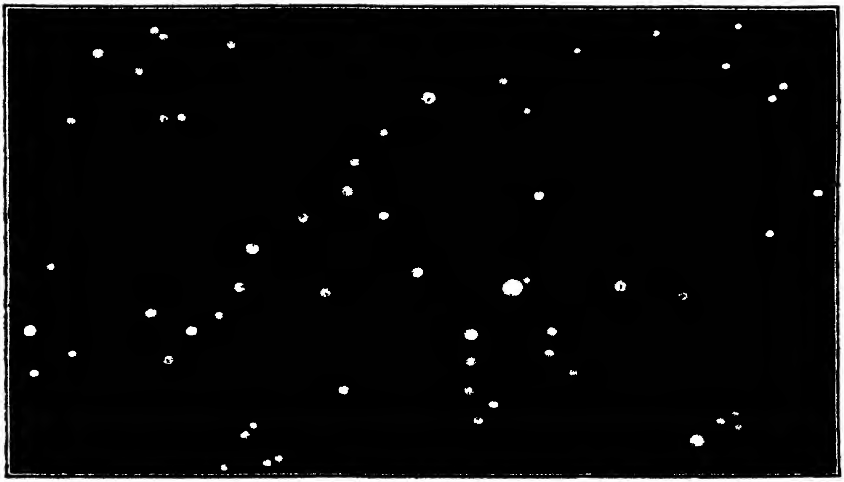
It is scarcely necessary to add that the largest telescope ever made by man—the great Parsonstown reflector—has revealed

* There is an interesting quotation at page 267 of Webb's *Celestial Objects*, in which Fr. Secchi describes the astonishment with which, when studying certain galactic regions, he saw spirals and curves of stars so regularly disposed as to preclude all possibility that chance distribution was in question.

even in the intricate constitution of the nebulæ the existence of streams and sprays, sometimes spiral, sometimes but slightly curved, sometimes disposed with singular regularity, at others extending in irregularly shaped branches, growing gradually fainter and fainter until they are at length lost altogether either by diffusion or through extreme faintness.

But it remains to be considered that we have strong evidence in favour of the view that the Milky Way itself is but a stream, or rather a congeries of streams of stars. The evidence on which Sir William Herschel rested his theory that the galaxy is of the shape of a cloven flat disc, was abandoned by himself

FIG. 5.



A portion of one of Chacornac's ecliptic charts, the centre being in 4½° S., and 23h. 26m. R.A.

during the later years of his career as an observer; and he recognised clearly that some of those rich nodules of the Milky Way which can be seen in the northern heavens are real aggregations of stars (not vast depths along which the stars are arrayed as in a sort of procession), and that such aggregations approach in figure to the spherical form. In the southern heavens Sir John Herschel recognised galactic regions to which Sir William Herschel's later mode of reasoning could be applied even more convincingly. Now precisely the same reasoning by which Sir W. Herschel was led to regard the rich clustering regions of the Milky Way in Cygnus as spherical in form, seems to show that the well-marked portions of the galactic stream are really stream-shaped. And this view of the galaxy, which might seem to agree ill with the usual

account of this marvellous band of stars, accords excellently with the description given by Sir John Herschel and others who have most carefully studied the galaxy. More especially is it suggested by the aspect of the Milky Way in the southern heavens. For there the continuity of the zone, on which so much stress had been laid is shown to be interrupted by a broad dark rift, a feature wholly inexplicable on the theory that the Milky Way is shaped like a cloven flat disc. And over the whole region from Argo, over the feet of the Centaur, to Sagittarius and Scorpio, the Milky Way as pictured by Sir John Herschel presents an appearance far more closely according with the theory that the Milky Way in this region forms a gigantic spray of stars than with any other that has been propounded. In the northern heavens, the faintness of the Milky Way causes it to appear more uniform in structure; but even in the northern heavens, as has been well pointed out by Professor Nichol, it is only on the most cursory examination, or when the Milky Way is studied under unfavourable circumstances, that it can be regarded as a simple zone. But it is well worthy of notice that in my chart of 324,198 stars, the Milky Way reveals itself (through the mere aggregation of stars down to the 9½th order) as a congeries of streams, with branching extensions, of which only the commencement can be recognised as more or less marked projections, in the best pictures of the northern parts of the Milky Way.

It remains to be noticed, in conclusion, that the nebular system also shows the most marked tendency to stream-formation when isographically charted, as in the series of charts which illustrate my paper on the distribution of the nebulæ, in Vol. xxix. of the Monthly Notices of the Astronomical Society. The tendency to stream-formation is more especially to be noticed among the southern nebulæ. It is worthy of remark that, whereas the southern nebular streams converge upon the Magellanic Clouds, the northern nebular streams seem to extend towards the outlying streams of the Milky Way, as it appears in my chart of 324,198 stars. The evidence of a real association between stars and nebulæ is singularly strengthened by these peculiarities of arrangement.

# REVIEWS.

## POPULAR SCIENCE.*

IT is wonderful, but still it is not the less true, that scientific readers, or readers of scientific books, are in a vastly larger proportion in the United States of America than they are here. It is perfectly amazing to be installed into the secrets of some New York publisher, and find that books, of which you hardly heard at home, treating on questions of special scientific importance, have had a sale in New York which is reckoned by thousands. Still, England is the producer, if she be not the reader, of scientific books; and in no instance is this fact more fully or admirably illustrated than in the case of the work under notice. Mr. Proctor is one of our best scientific writers, as perhaps many of the readers of this journal are aware already, but he is not only so in a truly scientific sense: he is not only thoroughly and remarkably accurate, but he possesses in a very marked degree that excellence and purity of style which are at once so attractive to the general reader, and so rarely met with in the scientific world. In the book now under notice, the reader, accustomed to Mr. Proctor's contributions to these pages, will be surprised to find that the writer has not confined his attention to purely astronomical subjects, but that physical geography, zoology, geology, physics, and physiology have each and all formed subjects of careful and advanced reading by the author. And we say this, not out of an empty desire to compliment an author who has been a contributor to our pages, but from the fact that many of the contributions, though written for some daily or other journal originally, bear on them the stamp of original thought and pure reflection. They are not essays such as we too frequently find in our journals, sparkling with bright writing, but devoid of anything like careful thought and reflection—productions which will not bear a moment's thought or reflection from the reader who is versed in his subject. Far from it, indeed. In some instances we have wondered to find them so very learned; and we have been surprised that so prolific a writer on astronomical subjects should have either the time or the inclination to go so fully into questions which have no real bearing upon the series of matters which he is engaged in studying. Of the truth of this

* "Light Science for Leisure Hours. A Series of Familiar Essays on Scientific Subjects, Natural Phenomena, etc. etc." By Richard A. Proctor, B.A., F.R.A.S. London: Longmans, 1871.

we could not have a better instance than that afforded by Mr. Darwin, in quoting from the article on "Death-Rate," in his last work on "The Descent of Man," and in mentioning Dr. Farr's special opinion as to the quality of the paper quoted from. Of the numerous chapters in the work it is impossible for us to speak specially: suffice it to state the subjects of a few. Thus we have "Strange Discoveries respecting the Aurora," "Our Chief Time-piece Losing Time," "Recent Solar Researches," "Secret of the North Pole," "A Great Tidal Wave," "Deep-Sea Dredgings," "Tunnel through Mont Cenis," "Earthquake in Peru," "A Shower of Snow Crystals," "Influence of Marriage on the Death-Rate." "The Safety Lamp," "Photographic Ghosts," "Betting on Horse Races," "Squaring the Circle," and a new theory of "Achilles' Shield." These are but a few of many articles, but they strike us as among the most important. Especially interesting, however, of all these is the paper on "Our Chief Time-piece Losing Time." This is both a remarkable and an interesting article. It relates to the fact that the motion of the earth is gradually altering, so slowly of course that it is almost imperceptible, but still decidedly. This change relates to the form of the earth's orbit, which, with regard to the motion of the moon, is decidedly undergoing a change. Of course this is an alteration of not the least importance in a practical point of view; but it is one which is of importance astronomically, and which is most marvellous as illustrative of the wonderful accuracy of modern research. "Suppose," says Mr. Proctor, "that, just in front of our moon a false moon, exactly equal to ours in size and appearance, were to set off with a motion corresponding to the present motion of the moon, save only in one respect, namely—that the false moon's motion should not be subject to the change we are considering, termed the *acceleration*. Then one hundred years would elapse before our moon would fairly begin to show in advance. She would in that time have brought only one one-hundred-and-fiftieth part of her breadth from behind the false moon. At the end of another century she would have gained four times as much; at the end of a third, nine times as much; and so on. She would not fairly have cleared her own breadth in less than twelve hundred years. But the *whole* of this gain, minute as it is, is not left unaccounted for by our modern astronomical theories. *Half* the gain is explained, the other half remains to be interpreted; in other words, *the moon travels further by about half her own breadth in twelve centuries than she should do according to the lunar theory.*" But what, we may ask, is the cause of this singular retardation, if so it be, of the earth's rotation-movement? The cause would appear to be, according to Mr. Proctor, the movement of the tides. And a little reflection will serve to convince anyone who doubts it that here is an expenditure of a very considerable degree of power, of indeed in some cases an enormous degree of almost unassailable force. Now where, asks Mr. Proctor, does this force come from? "Motion being the great 'force-measurer,' what motion suffers that the *tides* may work? We may," he thinks, "securely reply that the only motion which can supply the requisite force is the earth's motion of rotation." Therefore, he says, it is no idle dream, but a matter of absolute certainty that, though slowly, still very surely, our terrestrial globe is losing its rotation-movement. Other chapters in the

author's book are not less interesting than that from which we have quoted, but our limited space forbids our entering upon them. The book is full of instructive reading, and is withal a most attractive volume.

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## TERRESTRIAL MAGNETISM.*

WHATEVER may be thought of this author's views by physicists competent to follow him through a difficult line of argument in a subject where few ordinary persons are qualified for the task, there can be little doubt, we think, that he has performed his task ably. So far as we have examined his views, they are throughout fortified by an amount of learning and patience in investigation which might have made the author sufficiently courageous to have appended his name. And we think so all the more because we know how important is a name in making a book be read and carefully attended to. Of course, if the author bore a name unadorned or at least undistinguished in physics, it would be a different affair. It would cause his book to convince some persons of the error of his views. But really we do not think that this is the case. To us the author of the book appears, as no doubt he is, as a writer to whom physics has been a subject of careful and prolonged study, and who has by this time obtained distinction in his subject. However, be that as it may, there is no doubt that the theory he puts forward is modestly set forth and is further supported by a multitude of facts in the science of physics. It will of course yet remain undecided as to how our earth became originally magnetised; but, granting its magnetisation, we think it is much more coincident with facts as they are to suppose a condition of electricity intervening between the earth and the sun, and that this is the parent of the earth's magnetism, than that the latter should be derived from any kindred condition in the sun itself. Indeed, the author's illustration of the position of the earth in regard to the sun and the impossibility of its poles corresponding to the *two* of the earth, appears to us adequately clear. In fact, it seems probable, from analogy, that if the two poles should act on one of the earth's, they would thus destroy each other. Further, as the author has shown (and this seems to us to be a more powerful argument), the motion of the sun round its own axis should produce profound changes in the magnetisation of the earth every twelve-and-a-half days, or half the sun's own period of rotation. This and other facts which he adduces sufficiently, we think, disprove the view of the sun's direct activity as a magnetiser. In the further elaboration of his views we shall not follow the author. Nor do we desire to express any opinion on their accuracy or force. The subject is one which can only be followed out by an experienced physicist, and by him only with considerable time at his disposal. The book is well got up, and contains abundant maps and plans illustrating the variations

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* "A Treatise on Terrestrial Magnetism." Edinburgh: William Blackwood, 1871.

of the magnetic needle, *et hoc genus omne.* Altogether, the work is one which, for an anonymous contribution, we have to speak of in terms of high praise. We commend it to the consideration of our physical readers. It is well worthy their perusal.

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## ON SPONTANEOUS GENERATION.*

WE had almost thought that defenders of spontaneous generation were extinct. Of course we always except M. Pouchet, of France; but of late years he has done so little in this direction that he almost may be considered to have given up the battle, not convinced, of course, of the error of his ways, but believing rather in the unwisdom of his opponents, and especially of M. Pasteur. But during the past two or three years an English champion for this peculiar doctrine has sprung up, and, so far as we can see, he is a more formidable supporter of the doctrine than even M. Pouchet himself. It is necessary in cases of controversy on a scientific subject to look to the men holding the opposite sides; and when we do so in this instance, we see a formidable array of authorities going in against the author of the present volume. For example, we have in the first ranks of opponents Professor Huxley, who goes in strongly and determinately against the doctrine. Yet, strange to say, Professor Huxley would be, we should think, one of the first to admit that this process must have been at one time a distinct operation; for it is only by it that men of his school can suppose the origin of organic beings on the earth, unless indeed they take Professor Sir W. Thompson's rather singular aerolitic theory into consideration—an hypothesis which, after all, only pushes the difficulty back a little farther than it was before. Well, then, we have Professor Huxley distinctly in opposition to the doctrine; and this alone is very seriously against it, for Professor Huxley is not one who takes up an hypothesis without due consideration. If he has gone in against spontaneous generation, it is only after giving mature consideration to those experiments which have been recorded in its favour. He is, therefore, a tremendous opponent. Still, we must remember, on the other side is Professor Owen, who in his last volume goes in strongly and decidedly in favour of spontaneous generation. Between two such opponents, what is one to do? Clearly, authority is of no moment; for if Professor Huxley does not believe without careful investigation, surely we cannot accuse Professor Owen, the greatest worker in comparative anatomy which the world has ever produced, of carelessness or want of consideration in the belief which he so strenuously expresses.

But really it is almost impossible to decide the question at the present moment. And we think that before further investigations are made, it would be well to have some researches exactly carried out into the temperature which some of the lowest organisms will tolerate without perish-

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* "The Modes of Origin of Lowest Organisms; including a Discussion of the Experiments of M. Pasteur, and a Reply to some Statements of Professors Huxley and Tyndall." By H. Charlton Bastian, M.A., M.D., F.R.S. London: Macmillan & Co., 1871.

ing altogether, and into the question of when they are to be considered living or dead; for it is really on this point of temperature that most of the experiments are based. From a careful examination of Dr. Bastian's most interesting work, which contains a host of researches, we are much disposed to agree with him in his supposition that the process which he advocates is not only likely, but actually takes place. At the same time, we confess that there are obvious faults in the line of reasoning which Dr. Bastian pursues. He does not seem to us to have determined with sufficient clearness that the heat to which he exposed certain of his solutions was amply adequate to kill the genus which he caused to undergo their development. If this be so, it is clear that he has the most of the argument with Professor Huxley. It seems to us that he is right; but clearly, as the argument stands, Professor Huxley has the best of it. In some of his experiments Dr. Bastian does not appear to us to have been adequately careful. See, for instance, when he says (p. 63), "On the other hand, if the turnip solution be neutralised by the addition of a little ammonic carbonate, or liquor potassæ; or, better still, if even *half a grain of new cheese* (the italics are ours) be added to the infusion before it is boiled, then I have found that the fluid speedily becomes turbid, owing to the appearance of multitudes of *Bacteria*. In an infusion to which a fragment of cheese had been added I have seen a pellicle form in three days, which, on microscopic examination, proved to be composed of an aggregation of *Bacteria*, *Vibriones*, and *Leptothrix* filaments." Surely, it must occur to Dr. Bastian that the cheese which he introduced must have contained the parents of the bacteria produced, and that probably his boiling the liquor was not sufficient to destroy the life of their organisms. In any case, this, which is in some respects a type of the author's experiments, would not be sufficient to satisfy those who are sceptical upon the subject. We trust, therefore, that in the work which Dr. Bastian promises us on the subject more satisfactory investigations will be found. The present work is most interesting; but, as an argumentative one, it is not sufficiently conclusive.

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## NATURAL PHILOSOPHY.*

IT is, to a certain extent, unfortunate—we mean, of course, for the publishers—that there should be at the same time two series of works of the same kind for the use of students. Messrs. Longmans are issuing a series of excellent and cheap manuals upon the different branches of science, and almost at the same time the Messrs. Groombridge are advertising for sale a set of works, nearly of the same class, and appealing to exactly the same people. The present work is one of the Natural Philosophy Series, and is in every respect, so far as we have seen, an excellent manual. Its physical features, size, type, and illustrations are all excellent, and the general

* "A Course of Natural Philosophy, containing the Elements of Mechanics, Hydrostatics, and Optics, for Schools, &c." By Richard Wormell, M.A., B.Sc. London: Groombridge, 1871.

character of the work is very good. Of course, the subjects are all treated in the most elementary style; but we doubt not that, so far as it is intended, as the author implies, for matriculation examination of the London University, it is amply sufficient for its purpose. According to the author, it is introductory to his previous work for the subsequent examinations of the London University, while it follows the same form and order, so that students, in pursuing subjects further, will be able easily to connect what they have to learn with what they have already learnt. At the same time, the author states that the book "by no means represents the narrowest view which may be taken of the curriculum of the London University, but will, it is hoped, be found useful in schools generally, as containing a systematic explanation of the more elementary principles of this branch of natural philosophy." We notice that each chapter is succeeded by a long series of questions, which takes the pupil through examples of all he has been studying. In most instances these questions are followed by the answers. In some they are not, thus leaving a certain amount within the range of the student's knowledge. So far as we have seen, it is in all respects a clear and good manual.

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## PLANT-FOOD.*

THIS book, the author tells us frankly, lays no claim to originality. We are glad to hear it, for assuredly if it was not distinctly stated in the preface, we should have taken the author somewhat to task for his labours. But now the question comes, if there is no originality in the work, why was it published? To this we have no answer in the author's prefatorial remarks, and we are unable to guess anything in the shape of a reply ourselves. The book is essentially a made-up one; and is, in addition, spread over about double the quantity of space that is requisite. We do not well know what to say about such works. They are really without any distinct value, and usually—and indeed the present is no exception—they put in a very unsatisfactory and abstractive sort of way, what is much better put, if a little more lengthily, in a more important treatise on the subject. For example, when we have in English such a splendid treatise as the "Natural Laws of Husbandry," by Liebig, it is more than absurd to issue such a rudimentary incomplete work as that which Mr. Grundy has offered to the public. Of course it must be said that throughout his pages there is no symptom of grave error; the book is accurate so far as we have seen. But the idea of publishing such a rudimentary kind of book in the language which contains Liebig's splendid treatise, looks to our mind as a great weakness on the author's part. Such books can do little or no good; and, except that they gratify their author, we are ignorant of any service they can be capable of offering.

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* "Notes on the Food of Plants." By Cuthbert C. Grundy, F.C.S. London: Simpkin & Marshall, 1871.

## DESCHANEL'S NATURAL PHILOSOPHY.*

PROFESSOR EVERETT has done well to reproduce this volume from the French. Whatever the reader may think of his former attempt in the same direction, that is, however unsatisfactory he may deem it, he must be satisfied with the translation of the volume by Professor Deschanel on Heat. In the present work, however, we find that besides the author's labours being particularly good, the editor has laboured equally well, and has added a great deal so as to make the book a really good one on the subject of heat, and to leave in the student's hands a volume which he may read with satisfaction and profit. It must not be supposed from these remarks that the work is of a very advanced character, for it is not. But it bears evidence of being an exact treatise; and it contains abundance of matter, quite sufficient for the "medical" student or the first-year's-man in "Arts." The subject of the volume is Heat; and this is dealt with very fully, the chapter on Thermodynamics and large portions of the chapters on Conduction and on Terrestrial Temperatures being the work of the editor. The nomenclature of units of heat is borrowed from Professor G. C. Foster's article on Heat in Watts's "Dictionary of Chemistry," and several other shorter portions of the English edition have been added by the editor. For instance, the chapter on the motion of glaciers, where he adopts Forbes's views in preference to Tyndall's. This adoption of Forbes's theories is not usual, but we think it right; we fancy from Forbes's work upon the subject that he most conclusively proves the force of his ideas. On the whole, the book is a very good one, and we have much pleasure in recommending it.

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## BRITISH FUNGI.†

OF all the departments of Botany we fancy that that of the Fungi has been the least studied by the amateur, and, doubtless, the reason has been the absence of a suitable manual wherein the student could find every species which he was likely to meet. However, this need no longer exist, for Mr. M. C. Cooke, M.A., has supplied a work which will long outlive him, and which must for many years be regarded as the standard instructor on the subject. He has given us, in two volumes of more than 900 pages and with more than 400 well-devised woodcuts, an admirable account of all the British Fungi. It is true, as he himself admits, that in many cases the American distribution is imperfectly given, but this is a very small defect, if it be one at all, in such an admirable work. We think, too, that considerable praise is due to Mr. Macmillan for the admirable manner

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* "Elementary Treatise on Natural Philosophy." By A. P. Deschanel. Translated and Edited by J. D. Everett, M.A., D.C.L., Professor of Natural Philosophy in Queen's College, Belfast. London: Blackie, 1871.

† "Handbook of British Fungi." With full descriptions of all the species and illustrations of the genera. By M. C. Cooke, M.A. London: Macmillan & Co., 1871.

in which he has turned out the work, in every particular with which the publisher has to do. The volumes are such as to uphold the highest credit of the house that issues them. When we consider the fact that no book has been published on this subject for the last thirty-five years, we can imagine what a labour Mr. Cooke's has been; and when further we know that his volumes and illustrations have both far exceeded the mark which was originally fixed, we may form some idea of the absorbing nature of his work in producing them. The review of such a book is a task only for the botanist who has devoted a lifetime to the study, and he after all would be biassed in his selection of some particular mode of division. To ourselves the book seems admirably arranged, and the classification adopted particularly clear. Besides, with the help of the engravings, no one need—with a little caution—make any mistake in diagnosis. Fungi generally, or *Sporifera*, those plants which have the spores naked and with which the present work is especially connected, are divided by Mr. Cooke into *Hymenomycetes*, *Gasteromycetes*, *Coniomycetes*, and *Hyphomycetes*. With these four divisions his books are especially connected, of which the first of course occupies the greater part of volume one; the others dividing the rest of the book between them. So far as we have seen, the arrangement is usually simple, so many characters being employed as are absolutely necessary and no further, and thus the definition of any particular species, more especially of the *Hymenomycetes*, becomes a task of no great difficulty. The whole book is one, therefore, which we can in the highest degree speak well of; and while we beg to return our thanks to Mr. Cooke for the excellence of his labours, we cannot but recommend those of our readers who are interested in the study of Fungi, to purchase the volumes for themselves.

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## PLANE AND SOLID GEOMETRY.*

MR. H. W. WATSON has been selected as the author of Messrs. Longmans' "Manual of Geometry," and we think the selection has been extremely well made. Although in this little treatise before us there is not much that is not to be found in the work of Messrs. Ronché and Comberousse, there are in many instances detailed and important alterations which we fancy are to the reader's advantage. We notice as especially deserving attention, that a certain alteration has been made in the non-adoption in some instances of the syllogistic method. We think that when these changes have been made the author has shown considerable caution, and has not altered more than is absolutely necessary. Altogether, we consider the present work is every way as good as those in the same series which have gone before it.

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* "The Elements of Plane and Solid Geometry." By H. W. Watson, M.A. London: Longmans, 1871.

## A NEW VIEW OF CAUSATION.*

HERE is a little book by a London author, which may be read with advantage by those who are interested in philosophic treatises on the origin of things generally. It is, we think, tolerably clearly written, but it would have been better had the author given us a little more of himself and a little less of other authors. As it is, he occupies but a very small portion of a very small book, of about 150 exceedingly small pages, in excessively large type. His views are in most instances correct, and he aims at a proper mode of forming opinions.

## DARWINISM REFUTED.†

WE call attention to this very little book because it is one of the briefest, shallowest, and most ignorant treatises we have ever seen upon the subject. The author does not clearly understand the hypotheses of Mr. Darwin; and his ignorance of contemporary work is something very lamentable in one who proposes to write upon so important a subject as that which he has taken up. The mere statement that Sir Charles Lyell is opposed to Darwinism, is sufficient to show how conversant the author is with recent scientific labours. This book is a most ignorant and worthless production.

## THE SMITHSONIAN REPORT.‡

THIS Report for 1869 has just been published, and has the year 1871 upon its title-page. It contains, besides the monetary and other reports which are only of local interest, a number of very valuable papers, translations, &c. Among them may be specially mentioned Sir John Lubbock's Liverpool address on the "Lower Races of Man," Professor Huxley's splendid essay on the "Principles and Methods of Palæontology," which appeared in 1865 in a Catalogue of the Museum of Practical Geology, and M. Marey's splendid monograph, with over thirty engravings, and occupying nearly fifty pages of this work. There are several other important papers, which render this volume one of the best contributions to science which are issued during the year in America.

* "A New View of Causation." By T. S. Barrett. London: Provost & Co., 1871.

† "Darwinism Refuted: an Essay on Mr. Darwin's Theory of the Descent of Man." By S. H. Laing. London: Elliot Stock, 1871.

‡ "Annual Report of the Board of Regents of the Smithsonian Institution," for the year 1869. Washington: Government Printing Office, 1871.

## *SHORT NOTICES.*

*An Elementary Course of Theoretical and applied Mechanics, &c.*, by Richard Wormell, M.A., B.Sc. 2nd edition. London: Groombridge, 1871. This is the second edition of a work which we fancy we have had before under our notice. It is one of Messrs. Groombridge's series of educational manuals, and is a very good book of its kind. This, the second edition, contains some things not in the first one, and is altogether an improvement on it.

*Preliminary Report on the Vertebrata discovered in the Port-Kennedy Bone Cave*, by Professor C. D. Cope. This is a reprint, for private circulation, of a paper read by the Professor before the American Philosophical Society in April last. It deals very ably and minutely, as might be expected of its author, with the animals found in a fissure of the Potsdam limestone. The paper describes the several species, and concludes with some very curious observations on the former connection of the American and Asian continents.

*The Great Pyramid of Jizeh: Plan and Object of its Construction.* Cincinnati: Clark & Co., 1871. This is a curious pamphlet, which bears no author's name. Still the author has evidently read some of the works upon the subject; mainly, that by Professor Piazzi Smyth. He regards the Pyramid as a standard of measure; and gives numerous illustrations in proof of its being so.

*A Key to the Natural Orders of Wild Flowering Plants*, by Thomas Baxter, F.G.S. London: Simpkin & Marshall, 1871. This is a series of tables for the purpose of teaching botany to beginners. The author fancies it has certain features of simplicity; but for our part, we fail to see in what it is simpler than a good elementary treatise, such as Bentham's, for instance.

*On the Cause of Rain Storms, the Aurora, and Terrestrial Magnetism*, by G. A. Rowell. London: Williams & Norgate, 1871. This is a reprint of some old papers on a subject of considerable importance. They are written by an amateur; but, nevertheless, we think them worthy of a favourable opinion. We commend them, therefore, to the notice of our readers.

*The Natural History of the British Diatomaceæ*, by L. Scott Donkin, M.D. London: Van Voorst, 1871. Part II. The second part of this work has appeared at last, and without an apology for the delay, or a promise of better things in future. It deals with about forty specimens. The plates are only four in number, and we must say they are simply abominable. It is not at all to Mr. Van Voorst's credit, he who has published so many splendid natural history works, that this volume should be issued in its present form.

*George W. Childs: a Biographical Sketch*, by James Partin. Philadelphia: Collins. An interesting but brief sketch of a remarkable man.

*Digitalis and Heart Disease*, by Balthazar W. Foster, M.D., Professor of Medicine in Queen's College, Birmingham, and Physician to the Queen's Hospital. This is a pamphlet which is most creditable to its author. It is not of any great length, but it is carefully put together, and displays con-

siderable experience of cardiac disease. We cannot discover from what it is reprinted. Mayhap it is an original essay.

*The Triumph of Evolution, and other Poems*, by Joseph Merrin. London: Longmans, 1871. The title sufficiently conveys what this work is about. We pass no opinion upon it.

*A complete Course of Problems in Practical Plane Geometry*, by J. W. Palliser. London: Simpkin & Marshall, 1871. The lecturer on geometrical drawing at the Leeds School of Art and Science has here given what appears to be a useful work. It is accompanied by no less than 260 diagrams; and so far as we have seen, it is simple enough, provided the student goes on step by step through its pages. We think Mr. Palliser has done well to produce it. We fancy he has supplied a want much felt.

# SCIENTIFIC SUMMARY.

## ASTRONOMY.

THE *Eclipse of December* 12, 1871.—This eclipse will probably be well observed. We have already spoken of the track of the moon's shadow, but when our last Summary appeared it was not considered likely that an expedition would be sent out from England to take any part in the work of observation. Application has been made, however, to Government, and the sum of 2,000*l.* has been granted, as well as transport and the means of camping, &c., for an English expedition to Ceylon. As a strong observing party will proceed from Sydney and Melbourne to the stations in northern Australia, while the Dutch Government will probably garrison suitable observing stations in Java, there is abundant reason for believing that the eclipse will be well observed. Observations in India will probably be superintended by Mr. Posson (the Government astronomer at Madras), Colonel Tennant, and Lieutenant Herschel. It is hoped that Mr. Lockyer will be able to head the expedition to Ceylon. At the present time it is, indeed, understood that he will do so, in accordance with the request of the Astronomical Society (who have felt it incumbent upon them, we understand), to urge that one or other of our principal solar spectroscopists should take charge of the English expedition; but we can regard nothing as definitely settled at this moment. It is hoped that M. Janssen may be able to go to Java. We remind our readers that in North Australia the totality will last 4 m. 18 sec., or 2 m. longer than at the best stations last December. In Java the duration will be less, and further west in Ceylon the duration will be only a few seconds longer than in last year's eclipse. A slight mistake has been made on this point, however. Mr. Hind, in his first and comparatively rough account of the eclipse, had marked the shadow track as barely extending to Trincomalee. He now so places Trincomalee, with reference to the track of shadow, that he estimates the duration of totality there at 2 m. 30 sec.; and it has been inferred that, since this is the case with a town on the border of the shadow track, places in Ceylon near the middle of the track will have longer totality. But, as a matter of fact, Trincomalee is now shown to lie very near to the middle of the shadow's path. However, there can be no doubt that, with suitable observing weather, stations in Ceylon will have a considerable advantage over the best stations for observing last year's eclipse.

As to the observations which are to be made or which require to be made, we have a few remarks to make. Photography is to be applied again,

but under conditions which promise better success than has hitherto been obtained. In particular, advantage is to be taken of the experience acquired by Mr. Brothers last year. Mr. Brothers, it may be remembered, adopted a new method of photographing the corona, making use of an ordinary photographic lens of long focus instead of a telescope. Notwithstanding very unfavourable weather, the eclipsed sun having been clouded over until within a few seconds of the end of totality, Mr. Brothers succeeded, as we know, in picturing the corona "as it was never seen on glass before." It is not to be wondered at, therefore, that it has been decided to employ the same method on the present occasion. The proposed spectroscopic observations are also promising. Two excellent 6-inch refractors are to be mounted on the same equatorial stand, one at each end of the declination axis; so that, while one observer studies the aspect of a portion of the corona through one telescope, another may study the spectrum of the same portion through the other. This contrivance has been suggested by Dr. Huggins, and appears well calculated to remove the doubts which have hitherto rested on the subject of those spectroscopic observations which have been guided merely by means of the ordinary finder. Mr. Lockyer has made suggestions, among which the following may be noticed. "At each place," he says (i.e. India, Ceylon, and Australia), "the spectroscopes should be employed for half-an-hour (to be on the safe side) before totality, in scrutinising the crescent at its narrowest place, and the chromatosphere outside the following limb of the moon. At each place, as before defined, there should be a spectroscope with a finder and equatorial motion (or some equivalent arrangement) directed to the sun's centre, to record any changes which take place in the spectrum from, say, half-an-hour before to half-an-hour after totality, and also during totality, *bien entendu*. The relative darkness or brightness of the lines should be recorded every ten seconds. The spectroscope should have moderate dispersion, large object-glasses for collimator and telescope, and with focal length such that two or three degrees round the sun should be taken in (i.e. 1° or 1½° from the sun's centre), and a large field.". . .

Coming to the details of the expedition to Ceylon, Mr. Lockyer expresses the opinion that it need not exceed the following numbers:—

1 telescope-spectroscopic observer; 2 assistants.
1 photographer; 2 assistants.*
1 spectroscopic observer; 1 assistant.

Or 8 in all.

To one suggestion of Mr. Lockyer's we are compelled to take grave exception. He notes, among observations which he regards as comparatively unimportant, "sketching anything but the *changes* in the corona." It appears to us, on the contrary, that the changes in the corona are precisely the phenomena which are, in the first place, most difficult to delineate; and, in

* Mr. Lockyer remarks that, in his opinion, this duty may perhaps be entrusted to skilled sappers. But the history of all the recent eclipses shows that, of all the departments, the photographic is the one which requires to be entrusted to the most skilful hands. Very useful spectroscopic work has been done with very little preceding practice; but no good photographic work has yet been done save by experts.

the second, the least likely to prove instructive. We know as a matter of fact that, during the totality, and more especially at the beginning and end of totality, a variety of strangely changeful phenomena, resulting from the varying illumination of our atmosphere by the prominences, sierra, and the really solar portion of the corona, may always be noted. It would be a problem of extreme difficulty to determine the precise way in which these variations are caused; but there can be very little question that they are chiefly due to the varying amount of illumination just referred to, and to the rapid changes of temperature resulting from the passage of the moon-shadow athwart the upper regions of the air. To pay special attention to such changes is a course admirably calculated, perhaps, to throw light on meteorological questions; but it does not seem to promise any new information respecting the real solar corona. The most valuable information respecting this remarkable appendage of our sun would unquestionably be gained if one observer, not suffering his attention to be distracted by the wonderful spectacle produced by the changing illuminations of the upper air, could discriminate between the changeful phenomena of the eclipse and those features of the corona whose fixity pronounces them to be true solar phenomena.

*Detection of the Annual Parallax of a Planetary Nebula.*—Mr. Gill, of Aberdeen, well known as a careful observer, and as having executed some successful photographs of the moon, announced at the recent meeting of the British Association that he had detected a parallax of nearly two seconds in the case of the planetary nebula 37 H. iv., close by the pole of the ecliptic. He is desirous, however, of continuing his observations for yet another year before definitively announcing that the nebula has this large parallax. Should this result be confirmed, it would follow that this nebula is nearer to us than any of the fixed stars, or at least than any fixed star whose parallax has hitherto been measured. The result would be interesting, as confirming those doubts which have recently been expressed respecting the vast distances at which nebula have been long supposed to lie. It must not be forgotten, however, that Sir William Herschel, by whose observations these vast distances have been supposed to be established, was himself the first to express doubts on the subject. The planetary nebulæ, in particular, were among the objects which he judged, during the latter part of his career as an observer, to be much nearer than he had imagined when he enunciated his earlier but better-known theories. It is somewhat singular that the particular nebula which has thus been the first to reward the search for nebular parallax, was the first nebula which Dr. Huggins found to be gaseous.

A somewhat remarkable comment is reported to have been made by Professor Tait upon Mr. Gill's paper. That eminent mathematician remarked, according to the report, that Mr. Gill's observation, if established, would tend to overthrow the theory now universally accepted among astronomers respecting nebulæ,—to wit, "that nebulæ are the moving matter of stars; showers of stones, Sir W. Thomson would call them, at inconceivably vast distances." We had never heard that this theory had even been enunciated among astronomers, though we have had through our hands the proceedings of all the principal astronomical societies of Europe and America, and all the most important treatises on astronomy which have been pub-

lished during the last ten years. Surely, either Professor Tait or the reporters must have made some mistake.

*Further Observations of η Argûs and the Surrounding Nebula.*—Mr. Abbott submitted, two years ago, pictures of this interesting nebula to the Royal Astronomical Society, which seemed to show that the nebula had undergone important changes. He now sends a third picture, accompanied by comments on the criticisms to which his former paper was exposed. His first attack falls on Mr. Proctor, who, in an article in "Fraser's Magazine" for December 1868 (presenting Mr. Abbott's work in a very favourable light), had ventured to express doubts whether *the stars* in Mr. Abbott's drawing of 1868 had been actually copied from the view given by the telescope, &c. Mr. Abbott says that all his drawings "were carefully copied from the object as described in the 'Astronomical Register' for January 1869;" and he adds that "there is little doubt but that Mr. Proctor's views on the subject would be very much enlarged, if he had the opportunity of seeing the star and nebula as they appear at Hobart Town." Commenting on Mr. Abbott's letter, Sir John Herschel, who had been the first to express doubts as to the placing of the stars in Mr. Abbott's drawings, remarked that there was not one among all the stars delineated which he could identify with any of those laid down in his own drawings and catalogued positions. "The most superficial inspection suffices to show that there is no correspondence between us; and that Mr. Abbott's field of view of 1° 8′ in diameter differs as completely from a similar field in my monograph, having η Argûs near the centre, as if the telescope had been directed to quite a different part of the heavens. . . . This would, however, be of little moment, were it permitted to suppose that attention had been given only to the delineation of the nebula, and that the stars had been put down at random, or with little regard to their real configurations. Mr. Abbott, however, in the paper which accompanies his diagram, distinctly repudiates this supposition, and insists on the correctness of his representations of the stars in the field of view delineated; not, indeed, as micrometrically accurate, but as careful eye-drafts." Mr. Abbott having been communicated with as to the doubts thus renewed, replies, under date February 7, 1871, endeavouring to re-establish the accuracy of his draughtsmanship. Finally, the whole series of papers has been submitted to the searching scrutiny of the Astronomer Royal, who conceded the main points, namely, that the nebula has shifted its position, and that Mr. Abbott was the first to announce this interesting fact, yet comments, in somewhat severe terms, on Mr. Abbott's drawings. "When we look closely to fundamental points," says Professor Airy, "all is confusion. Mr. Abbott's observations were all made with a refracting telescope, so that the order of the four cardinal points on the map would be $\mathrm{E}{}^{\mathrm{N}}_{\mathrm{S}}\mathrm{W}$ (turned round in in any degree). But in the 1870 map he has them marked $\mathrm{W}{}^{\mathrm{N}}_{\mathrm{S}}\mathrm{E}$; in the 1871 map he has marked only $\frac{\mathrm{N}}{\mathrm{S}}$. The $\frac{\mathrm{N}}{\mathrm{S}}$ in the two maps differ about 45°. In each of these maps is a line (different for the two) which he calls 'Line of Light.' What this means I have not the slightest idea. In points of geometry, therefore, Mr. Abbott is a most inaccurate man. As

regards the delineation of the nebula, I cannot make out anything. It is impossible for us to publish maps in this state." We have spoken of Professor Airy's examination of the subject as final; but as a matter of fact, a letter from Captain Herschel reopens the very question on which the Astronomer Royal had expressed himself satisfied. "Mr. Abbott appears to have got wrong," says Captain Herschel, "in his N. and S. points. Is it rash to suspect that he has also mistaken $\eta$? Neither does he appear to have even recognised the lemniscate. He speaks of 'a dispersion of the stars'; but his own drawing, as I now show, places most of his stars in approximately their right relative places. *Surely all this betokens non-recognition, on his part, of the object he was examining,* due probably to an inferior magnifying power. If his chart of the stars is as correct as I think, every atom of evidence of change in the nebula which *he* adduces is swept away."

To quote the remark with which Professor Airy opens his paper, "The subject is a very puzzling one."

*Proposal for a Series of Surveys of the Star Depths.*—Mr. Proctor, in a communication to the Royal Astronomical Society, indicates the necessity of a series of systematic surveys of the heavens, on a principle quite different from that on which the Herschels gauged the star depths. A series of telescopes of gradually increasing aperture should be employed to gauge every portion of the celestial sphere, the series of gauges for the several apertures being then charted isographically. His opinion of the value of such surveys is founded on the interesting results which are established by the isographic charting of all Argelander's series of 40 full-sheet charts, showing the places of 324,198 stars. It would not be necessary, however, to mark in every star separately, with careful reference to position and size, as in the isographic copy of Argelander's charts; all that would be necessary would be to mark in the observed number of stars (as determined by the gauges) in the corresponding spaces in the chart. The gauge fields should not be circular, but square (except close by the poles), so as to leave no ungauged spaces, and to avoid overlaps. By taking apertures of 3, 4, 5, 6, 8, 12, and 18 inches, or even to 2 ft. and 4 ft., a progressive series of charts would be obtained, which would throw great light on the laws of stellar distribution.

*The Physical Changes of Jupiter.*—Mr. Ranyard has contributed an interesting paper on this subject to the "Monthly Notices" of the Astronomical Society. He shows that, within two years of the great sun-spot maximum of 1848, the white spots on the southern belt were strongly marked, and the equatorial region much broken up. Within a year of the next sun-spot minimum, Mr. De la Rue made his large and well-known drawing of Jupiter with a 13-inch silver speculum. "It is full of the smallest details in the belts, yet there are no traces to be found of Dawes' markings, bright points, northern or southern eggs (*sic*), or equatorial port-holes." (We take some exception to these terms, as thus used absolutely; though they are very suitable expressions for comparative description.) Drawings made by Piazzi Smyth in 1856 entirely endorse Mr. De la Rue. But in 1858, when the sun was again marked with spots, Lassell noticed a numerous group of white spots in the bright equatorial region of Jupiter. "For several years," says Mr. Lassell, "I failed to see any such spots upon the face of Jupiter at all, but last year they appeared again in the same quarter of the

planet, and were attentively and ably observed by Mr. Dawes." In 1859 the flocculent cloudy appearances resembling port-holes in the principal belts were beautifully drawn by Sir J. Keith Murray. In 1860-61 again, Professor Airy, speaking of the great equatorial, says, "It has been employed to a considerable extent in the preparation of delineations of Jupiter and Mars. The former of these planets has exhibited in the last year some appearances never before recorded; and it has appeared very desirable to register as soon as possible anything which seems to indicate a change in the constitution of that great body." Mr. Carpenter therefore made, during that year, a series of most careful drawings, showing all the flocculent port-holes and the reddish colour of the equatorial region, bright egg-shaped spots, and elliptical markings, which have been observed during the last two years.

An important feature in Mr. Ranyard's paper is the fact that the evidence he adduces is taken from the recorded observations of Lassell and De la Rue, who are now the chief opponents of the assertion that Jupiter has lately changed in aspect, and, from the work done by the Greenwich equatorial, now said to give no evidence of change.

We may note in this connection that the picture of Jupiter at p. 280 of the last number of the *Popular Science Review*, has been inverted by mistake. The inversion is of no consequence in itself, since it simply causes the picture to present Jupiter in his natural instead of his telescopic position; but as the comparison between this picture and others might lead to confusion, it is desirable that the astronomical reader should be made aware of the mistake. (Mr. Webb did not see the proof *in situ*, and is therefore not responsible for the error.)

*Discovery of another Asteroid.*—Another asteroid, the 115th, has been discovered by Mr. Watson, of Ann Arbor, U.S., who bids fair to rival the most successful asteroid seekers.

*The November Shooting-stars.*—We remind our readers that the November shooting-stars should be looked for in the early morning hours of November 13 and 14. It is not likely that a display of a marked character will be witnessed, but considerable interest attaches to the determination of the relative richness of different portions of the system. It is not unlikely that stragglers, really belonging to the same system, may be seen (and known by their "radiant point") on several days before and after November 13.

*The Planets of the Quarter.*—Venus will be a morning star, attaining her greatest brilliancy on November 1, and reaching her greatest westerly elongation on December 6. Jupiter will be well placed for observation during the latter half of the quarter. Mars and Saturn will not be well placed for observation.

## BOTANY.

*Transpiration of Watery Matter by Leaves.*—Professor M'Nab of the Royal Agricultural College, Cirencester, has published an important and lengthy paper on this subject in the "Transactions of the Botanical Society of Edinburgh." The conclusions at which the Professor arrives may thus be

briefly stated:—1. Total quantity of water in the leaves of the bay laurel, 63·4 per cent.* 2. Quantity of water which can be removed from the leaves by calcium chloride, 5·08 p. c. 3. Quantity of water which can be removed from the leaves by sulphuric acid *in vacuo*, 6·09 p. c. 4. Quantity of water which can be removed from the leaves in the sun, 5·8 p. c. 5. Amount of transpirable fluid in stem and leaves, between 6 and 7 p. c. 6. Amount of fluid in relation to cell sap, between 56 and 57 p. c. 7. Rapidity of transpiration in sunlight, 1 hour, 3·03 p. c. 8. Rapidity of transpiration in diffused daylight, 1 hour, 0·59 p. c. 9. Rapidity of transpiration in darkness, 1 hour, 0·45 p. c. 10. Amount of transpiration in darkness, 48 hours (mean), 13·47 p. c. 11. Amount of fluid transpired in a saturated atmosphere, in sun, 1 hour, 25·96 p. c. 12. Amount of fluid transpired in a dry atmosphere, in sun, 1 hour, 20·52 p. c. 13. Amount of fluid transpired in a saturated atmosphere, in shade, 1 hour, 0·00 p. c. 14. Amount of fluid transpired in a dry atmosphere, in shade, 1 hour, 1·69 p. c. 15. Quantity of water taken up by leaves when immersed in it, 1½ hour (mean), 4·37 p. c. 16. Quantity of watery vapour absorbed by leaves in a saturated atmosphere, 18 hours, 0·00 p. c. 17. Amount of fluid transpired by upper surface of leaf, in sun, 1 hour, 1·34 p. c. 18. Amount of fluid transpired by under surface of leaf, in sun, 1 hour, 12·33 p. c. 19. Amount of fluid transpired, both sides coated with collodion, in sun, 1 hour, 0·96 p. c. 20. Amount of fluid transpired by upper surface of leaf, 48 hours in diffused light, 2·82 p. c. 21. Amount of fluid transpired by under surface of leaf, 48 hours in diffused light, 16·08 p. c. 22. Amount of fluid transpired, both sides coated with collodion, 48 hours in diffused light, 2·56 p. c. 23. Relation of fluid taken up to that transpired, and that retained by plant in 1 hour sunlight:—

| | Grammes. |
|---|---|
| Total amount taken up . . . | 1·088 |
| Deduct . . . | 1·038 |
| Difference . . | 0·05 |
| Amount transpired . . . . . | 0·64 |
| Gain of weight of branch . . | 0·398 |
| Total . . . . . | 1·038 |

24. Increase of weight of branch in saturated atmosphere, diffused daylight, 48 hours, 7·34 p. c. 25. Increase of weight of branch in ordinary atmosphere, diffused daylight, 48 hours, 7·14 p. c. 26. Increase of weight of branch in ordinary atmosphere, darkness, 48 hours, 3·01 p. c. 27. Rapidity of ascent of fluid in plants (*a*) $8\frac{7}{12}$ inches in 70 minutes, in sun. Lithium citrate. Transpiration equal to 7·58 p. c. per hour, in sun. Lithium all through branch. 28. Rapidity of ascent of fluid (*b*) $9\frac{4}{12}$ inches in 30 minutes; Lithium citrate. 29. Rapidity of ascent of fluid (*c*) $5\frac{3}{12}$ inches in 30 minutes; Thallium citrate.

*The Function of Bog Mosses.*—Dr. Braithwaite, in an interesting paper on mosses in the "Monthly Microscopical Journal" for July, says that with

* Percentage calculated on the total weight of leaves or branch employed.

respect to the function of these mosses, he cannot do better than quote Professor Schimper's words. He says:—"Unless there were bog mosses, many a bare mountain ridge, many a high valley of the temperate zone, and large tracts of the northern plains, would present a uniform watery flat, instead of a covering of flowering plants or shady woods. For just as the Sphagna suck up the atmospheric moisture, and convey it to the earth, do they also contribute to it by pumping up to the surface of the tufts formed by them the standing water which was their cradle, diminish it by promoting evaporation, and finally, also by their own detritus, and by that of the numerous other bog plants to which they serve as a support, remove it entirely, and thus bring about their own destruction. Then, as soon as the plant detritus formed in this manner has elevated itself above the surface water, it is familiar to us by the name of turf, becomes material for fuel, and all Sphagnum vegetation ceases."

*The Cross-Fertilisation of Scrophularia nodosa.*—It is probable that the dichogamy of the flowers of *Scrophularia* has already been observed and published, but it was new to Professor Asa Gray until pointed out this season by his assistant, Dr. Farlow. The arrangement is thus:—In the freshly opened blossom the upper part of the style is bent forward so as to bring the stigma, now ready for pollen, just over the patent lower lip of the corolla: the anthers, not yet dehiscent, are out of sight toward the bottom of the corolla, the filaments being strongly recurved or doubled over. In the blossom a day or two older, the stigma has dried up, the style become flabby, and the filaments have straightened so as to bring the four anthers up to the gorge of the corolla at the base of the lower lip, just back of the now withering stigma. The transversely dehiscent anthers are now widely open. The flowers are visited by honey-bees, which barely insert their heads into the gorge of the flowers; the chin or throat of the bee, coming into contact with the lower lip of the corolla, is necessarily dusted with pollen from the older flowers; and this pollen, in the passage from flower to flower, and plant to plant, is inevitably applied to the stigma of the freshly-opened flowers, which alone is in condition to receive it. The nectar sought by insects is here secreted abundantly by the corolla, at its base on the posterior side, and to some extent by the disk which girts the base of the ovary. The posterior face of the scale which represents the anther of the fifth stamen is apparently glandular, but hardly, if at all, nectariferous. Bees plunge their proboscis to the bottom of the flower.—*Silliman's American Journal* for August.

*So-called Mimicry in Plants.*—Professor Dyer read a paper on this subject before the British Association at Edinburgh. He pointed out the broad distinction existing between the mimicry of animals and what is called by that name in the vegetable kingdom. In the first case, the animal and what was mimicked were always found in close association. On the other hand, the plants in which a mimetic resemblance was observed were seldom found in the same neighbourhood. A striking resemblance in foliage would be found between a plant of the group of leguminosæ and another belonging to that of compositæ; as also between distinct varieties of ferns, though existing under entirely different conditions and indigenous to widely-separated portions of the globe, and in the leaves of several species of

caducous forest trees. The cause he attributed to the action of similar chemical agents on the structure of plants, so that those growing on the arid soil of the sea-coast might, from deriving nourishment from similar substances, come to have a similar form to plants of a totally different species, whose chosen habitat was high sandy regions. In the discussion which followed the reading of the paper, Professor Lawson demurred to the term mimicry being in any case applied to the resemblance observed in plants, that term inferring the existence of an intelligence which was the attribute of animal life alone; while Dr. Lankester protested against the supposition that the mimetic changes and resemblances observed in animals were the results of an exertion of the intelligence of the animal itself, or of anything but the peculiar conditions under which it was placed.

*The earliest Coniferous Tree. Dr. Dawson and Mr. Carruthers.*—Dr. Dawson has addressed a short note to the "American Naturalist" for June, stating, with reference to a notice copied in the May number of the "Naturalist" from the "Academy," that the opinion respecting the plant above named attributed to Mr. Carruthers is an entire mistake. "*Prototaxites Logani* is an exogenous tree, with bark, rings of growth, medullary rays, and well-developed, though peculiar, woody tissue; and, if Mr. Carruthers has made such a blunder as that attributed to him, this can only be excused by defective observations or imperfect specimens."

*Darwin's Theory applied to Plants.*—An excellent paper on this subject has been reproduced from the German in the "American Naturalist" for July. It is a very lengthy paper, and is abundantly supplied with notes. It is translated by Mr. Packard, jun., whose name is so well known here and in America. The original authors are Dr. E. Müller and Professor F. Delpino. It is the most important botanical contribution which has appeared for years.

*Fungi within the Thorax of Birds.*—At the British Association at Edinburgh, Dr. Murie referred to the circumstance of lowly organised vegetable structures being not unfrequently found growing in animals and man, both externally and internally. For the most part these affected the skin, giving rise to several cutaneous diseases. They also flourished in the alimentary canal; and among others, one peculiar form (*Sarcina*) had been described by the late Professor Goodsir from the human stomach. In nearly though not all instances where vegetable organisms flourished within the living body, it was in organs where a certain amount of air had free access. It was more difficult, though, to account for the cases where vegetable parasites arose in, so to speak, closed cavities. The instances which he (Dr. Murie) brought forward as coming under his own observation were three in number—viz., a fungus-like growth in the abdomino-pleural membrane of a kittiwake gull, a great white-crested cockatoo, and a rough-legged buzzard. After a general description of the specimens in question, the author referred to them as in some ways bearing upon those doctrines whereby living organisms were supposed to originate out of the tissues themselves. Other weighty reasons undoubtedly might be given to the contrary, but as every fact, either furnishing doubtful evidence of, or opposed to the spontaneous generation theory, might be useful at the present juncture, he (Dr. Murie) thought a record of such worthy of being brought before the Association.

## CHEMISTRY.

*Statue of the late Master of the Mint.*—Mr. William Brodie, R.S.A., has just completed the full-sized model in clay of a colossal statue, to be erected in Glasgow, of the late Thomas Graham, Master of the Mint. Dr. Graham's researches and discoveries in chemistry are known to all scientific inquirers. To general readers it may be necessary to mention that Dr. Graham was a native of Glasgow, being born in that city in 1805. At the age of twenty-five he became Professor of Chemistry at the Andersonian University; and in 1837 he succeeded Dr. Turner as Chemical Professor in University College, London, which position he held till 1855, when he was appointed Master of the Mint. In the same year he was created an honorary D.C.L. by the University of Oxford; he was also a Fellow of the Royal Society, and a Corresponding Member of the Academy of Science of the Institute of France. His "Elements of Chemistry" is a standard class-book, and he is known also as the author of a number of scientific papers. The memorial about to be erected in his native city is due to the gratitude and munificence of an old pupil, a wealthy Glasgow gentleman. The statue is to be cast in bronze, and is to occupy a position at the south-east corner of George Square, corresponding with that of Chantrey's celebrated figure of James Watt at the south-west corner.

*The compound Ammonium Amalgam formed by the Battery.*—In a late number of "Silliman's American Journal," the late C. M. Wetherill gives an account of the above. He says that a piece of filter paper was placed upon a glass plate, then saturated with a strong solution of the re-crystallised methyl ammonium oxalate. A globule of mercury of the size of a small pea was placed upon the paper, with the negative pole of twenty cells Bunsen in contact with it, the positive pole touching the paper. The globule swelled slightly, presented a buttery appearance, attached itself to and amalgamated the blade of a penknife which was in contact with the negative pole, and upon being pressed under a glass plate *showed innumerable gas bubbles in its substance* (in fact was a metallic froth), which emitted an ammoniacal odour. He promised other papers dealing more at length with the subject, but we suppose they were not done before his decease.

*Cobalt Ultramarine.*—In the "Journal für Prak Chemie" [Nos. 8, 9], Herr W. Stein contributes a paper on this subject. The author first refers briefly to his researches on ultramarine (see "Chemical News," vol. xxiii. pp. 119, 142, and 204), reminding his readers that a blue colour may result from the intimate mixing of a black and a white molecule. Next this paper contains researches on a sample of cobalt ultramarine, which had been kept for some twenty years in the museum of the Dresden Polytechnic School. The result of the analysis of that substance led to the following composition, in 100 parts:—Silica, 4·0; alumina, 68·45; cobalt (metal), 20·80; oxygen 6·75. The composition of the oxide of cobalt present in this ultramarine is $4CoO.Co_2O_3$. The analysis was confirmed by the synthesis of an ultramarine cobalt—viz. by simply igniting the black oxide of cobalt of commerce with alumina.

*Improvements in Chlorimetry.*—Mr. J. Smith, M.A., read a paper before the British Association at Edinburgh on "Improvements in Chlorimetry." He showed that the use of the milky solution of bleaching powder in chlorimetry is unsatisfactory, and hence it was necessary to discover a method of securing a clear solution containing all the chlorine by dissolving it in an alkaline solution. This is conveniently done by adding, say, ten grammes of bleaching powder to twenty grammes of soda crystals, filtering out the precipitated carbonate of lime, which is known to be washed, when it no longer discharges the colour of dilute sulphate of indigo, and making up the filtrate by water to one like of fluid. It is a clear colourless liquid of the specific gravity of 1·007, but if made of specific gravity 1·233 slightly greenish, having a pleasant oily feeling between the fingers, contrasting favourably with the roughness of the decanted solution of the bleaching powder with which it gives a precipitate. Most satisfactory results are obtained from it by all the chlorimetrical methods, and it has the additional advantage of showing the amount of lime in the sample by adding a solution of known strength of carbonate of soda until a precipitate is no longer formed.

*Dichroism of the Vapour of Iodine.*—Professor Andrews read a paper before the British Association on "The Dichroism of the Vapour of Iodine." The fine purple colour of the vapour of iodine, he explained, arises from its transmitting freely the red and blue rays of the spectrum, while it absorbs nearly the whole of the green rays. The transmitted light passes freely through a red copper or a blue cobalt glass. But if the iodine vapour be sufficiently dense, the whole of the red rays are absorbed, and the transmitted rays are of a pure blue colour. They are now freely transmitted as before by the cobalt glass, but will not pass through the red glass. A solution of iodine in sulphide of carbon exhibits a similar dichroism, and, according to its density, appears either purple or blue when white light is transmitted through it. The alcoholic solution, on the contrary, is of a red colour, and does not exhibit any dichroism.

*The Phosphate Process with Sewage.*—Messrs. Forbes and Price described their process to the British Association. This process, it was stated, was in operation at Tottenham. The sewage, after passing through some depositing tanks which had been constructed for the lime process, was pumped up at the rate of 800 or 1,000 gallons per minute along a carrier into a tank a hundred yards long, and of gradually increasing breadth. This tank took three hours to fill. As the sewage passed along the carrier the chemicals were mixed with it thus:—Two boxes were placed over the carrier—one a few yards further along it than the other; the first contained the phosphate mixture, and the second milk of lime. Men were continually stirring the contents of each box, which were allowed to run continuously into the sewage as it passed beneath the boxes. The amount of the preparation added was not ascertained, but it was stated to be certainly much less than the proportion indicated by previous experiments (one ton to 500,000 gallons of sewage).

## GEOLOGY AND PALÆONTOLOGY.

*Discovery of the Opercula of Hyolithes.*—Mr. W. S. Ford announces this discovery from New York, in "Silliman's American Journal." He says that several weeks ago, being in Montreal, he showed Mr. Billings, Palæontologist of the Geological Survey of Canada, a small collection of fossils that he had made in the Primordial rocks near New York. He pointed out to him, that among them there were the opercula of two species of *Hyolithes.* One is a minute circular species with four pairs of lateral muscular impressions, and two smaller, dorsal, all radiating from a point near one side. The other species is larger and like a *Discina* on the outside. Mr. B. showed him several specimens of the smaller species, that had been collected by Mr. T. C. Weston of the Canadian Survey last summer, in rocks of the same age below Quebec. He is informed that this is the first discovery of the opercula of *Hyolithes* yet made on this continent. He has made some observations on the rocks of this vicinity, and collected a number of species of fossils, of which he hopes to give an account at an early date.

*The Geology of the Rocky Mountains.*—We learn from one of our American contemporaries published in August, that Professor Marsh of Yale College, with twelve other gentlemen, has started for the Rocky Mountains and Pacific Coast. He will be absent until winter, and will continue his investigations of the Tertiary and Cretaceous formations which his explorations last year proved to be very productive in new species of vertebrates.

*What is Coal?*—This question is answered in a very able paper by Professor Dawson, LL.D., in the "Monthly Microscopical Journal" for August. He says that—1. The mineral charcoal or "mother coal" is obviously woody tissue and fibres of bark; the structure of the varieties of which and the plants to which it probably belongs, he has discussed in another paper. 2. The coarser layers of coal show under the microscope a confused mass of fragments of vegetable matter belonging to various descriptions of plants, and including, but not usually largely, sporangites. 3. The more brilliant layers of the coal are seen, when separated by thin laminæ of clay, to have on their surfaces the markings of Sigillariæ and other trees, of which they evidently represent flattened specimens, or rather the bark of such specimens. Under the microscope, when their structures are preserved, these layers show cortical tissues more abundantly than any others. 4. Some thin layers of coal consist mainly of flattened layers of leaves of *Cordaites* or *Pychnophyllum.* 5. The Stigmaria underclays and the stumps of Sigillaria in the coal roofs equally testify to the accumulation of coal by the growth of successive forests, more especially of Sigillariæ. There is, on the other hand, no necessary connection of sporangite beds with Stigmarian soils. Such beds are more likely to be accumulated in water, and consequently to constitute bituminous shales and cannels. 6. Lepidodendron and its allies, to which the spore-cases in question appear to belong, are evidently much less important to coal accumulation than Sigillaria, which cannot be affirmed to have produced spore-cases similar to those in question, even though the observation of Goldenberg as to their fruit can be relied on; the accuracy of which, however, he is inclined to doubt.

*South of Scotland Silurians.*—Mr. J. D. Brown read a paper on these before the British Association at Edinburgh. The object of the paper was to show that the silurian rocks of the south of Scotland, as developed in Dumfriesshire and Peeblesshire, are not all one geological epoch, as has been hitherto supposed, but belong to two different epochs—a lower one represented by the Moffat rocks, well known by their beds of anthracite shales and graptolites, and an upper series of later age, which lie unconformable on the Moffat rocks. These beds have been long known at Wrae and Glen Cotho, and more recently in Galashiels, through the exertions of Messrs. Lapworth and Wilson.

*New Fossil Reptiles from the Cretaceous and Tertiary Formations.*—Professor C. O. Marsh describes several of these remains in "Silliman's American Journal" for June. The remains were collected by the Yale College party during their explorations last summer in the Rocky Mountain region. The specimens from the Cretaceous formation are of great interest, as they further illustrate the remarkable development in this country, both in numbers and distinct forms, of the Mosasauroid Reptiles, which appear to have been comparatively rare in other parts of the world. Fortunately, moreover, some of these remains serve to clear up several obscure points in the structure of these reptiles, and prove conclusively that they had a well developed pelvic arch and posterior limbs; although up to the present time no satisfactory indication of this had been discovered, and the eminent palæontologists who have recently made these animals an especial study consider them probably destitute of these extremities. The remains found in the Tertiary deposits are also of importance, since they show that types of reptilian life, almost unknown hitherto from that formation in the West, were, in one of the ancient lake basins at least, abundantly represented there during that period.

*A huge Pterodactyl.*—We learn from the "American Naturalist" (July) that Professor Marsh states that the Yale College party obtained, in addition to the cretaceous fossils already described, several specimens which indicate a huge flying reptile, which he names Pterodactylus Owenii. The bones discovered "indicate an expanse of wings not less than twenty feet." The remains were found by Professor Marsh in the upper Cretaceous formation of Western Kansas. This is the first occurrence of the pterodactyl in America.

*The Sivatherium Giganteum.*—Dr. J. Murie, F.Z.S., read a paper before the British Association at Edinburgh, stating the systematic position of the extinct Sivatherium Giganteum, in relation to the deer, antelope, and other animals of the same species. The author introduced the paper by some remarks concerning the labours of the late Dr. Falconer and Sir Proby Cautely. These eminent men, the former a distinguished graduate of the Edinburgh University, brought to light, in their researches of fossil fauna of the Sewalik Hills, several remarkable mammalian forms. The sivatherium, one of these, as attested by its remains, must have attained the size of a full-grown elephant. It appears, however, to have been a ruminant. In some respects deer-like, in others more resembling the antelopes, still stranger it seems to have had some of the characteristic features of pachyderms, the tapir for example. Dr. Murie went on to show that it belonged to those

radical forms which by some may be regarded as one of the progenitors of diverse herbivorous groups. The sivatherium, according to him, is unlike all other living ruminants but one—the prongbuck—from the fact of its having had hollow horns, evidently subject to shedding. It differs thus from deer whose solid horns annually drop off, and from the antelope tribe, sheep, and oxen, whose hollow horns are persistent. Save one living form —the saiga—no recent ruminant possesses, as did the sivatherium, a muzzle resembling in several ways the proboscis of the tapirs and elephants. On the strength of his own recent researches, and of those of Mr. Bartlett and Dr. Caulfield, Dr. Murie is inclined to place the sivatherium in the family Antilocapridæ. Radiating from the sivatherium can be traced differentiation of structure allying it to the ancient bramatherium and megacerops. Diversely, links lead through the prongbuck towards the deer, giraffe, and camel. On the other hand, configurations point undoubtedly to the saiga, and there it is, as it were, split into lines directed towards the antelope, the sheep, and even the pachyderms.

*Flint Implements in Joshua's Tomb.*—The Abbé Richard gave an account of this recent discovery to the British Association at Edinburgh. He thinks that it upsets all geological hypotheses as to man's size. The Abbé unfortunately forgets (1) that the tomb may not be that of Joshua at all, and (2) that, if it be, there is no evidence as to the time the flint weapons were placed there. In fact they may have been, and probably were, there many ages before Joshua was placed there.

*Bats of the present and of the Mammoth Periods.*—Professor Van Beneden read a paper, in French, with this title, before the British Association. So far as was gathered, it was an argument against the Darwinian theory.

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## MECHANICAL SCIENCE.

*The Rhysimeter.*—Under this name, Mr. A. E. Fletcher has described, to the British Association, an instrument for measuring the velocity of streams of water, or the velocity of a ship relatively to the water through which it is moving. The apparatus is very simple. A double tube, with two orifices at the bottom, one of which faces the source of the current, while the other faces the opposite direction, is held in the stream, and communicates by tubes with the indicator where the pressure is measured, by columns of ether, water, or mercury, according to the circumstances of the case. The rhysimeter is already employed on some of the mail packets running to the United States, in place of the patent log, to ascertain the speed of the ship. It is more convenient, as giving the speed directly, without a time observation. The principle of the instrument is not new, but the construction is an improvement on previous instruments.

*Light Railways.*—Mr. W. Lawford has read a paper on this subject before the Institute of Mechanical Engineers. By *light railways*, Mr. Lawford understands railways of the ordinary narrow gauge, constructed as branches from existing trunk lines, but intended to be worked with light, flexible rolling stock, and at slow speeds. With a maximum load of 5 tons on one

pair of wheels, such railways might be constructed for 3,000*l.* to 3,500*l.* per mile of single way. Mr. Lawford described a short line of this description, constructed for the Duke of Buckingham and Chandos at Wooton. This line, of 6 miles in length, with a branch of 1½ miles, is essentially a surface line, the highest embankment being 12 ft. and the deepest cutting 10 ft. Turnpike and other roads are crossed on the level. The rails are 30 lb. bridge rails on longitudinal timbers. The ballast is 10 ft. wide and 6 to 9 in. thick under the sleepers.

*Compound Engines of the "Tenedos."*—In the recent trial of the engines of the "Tenedos," the consumption of fuel, in the six hour runs, varied from 1·58 lbs. per I. H. P. per hour, at eight-knot speed, to 2·32 lbs., at thirteen-knot speed. The rate of expansion of the steam was 9 times in the former case and 6½ times in the latter case. These results are extremely satisfactory.

*Water Power.*—The Industrial Society of Mulhouse has recently received communications about a project for utilising the fall of the Rhone at Bellegrande. According to the calculations of M. Colladon, of Geneva, this fall of thirteen metres could be utilised so as to afford 10,000 horses power. An American company, employed in the production of phosphate of lime, proposes to construct a tunnel for utilising this fall, and offers to Alsatian manufacturers to erect at Bellegrande establishments similar to those they possess at present. (Paris Correspondent of Engineering.)

*Self-acting Rudder.*—At the International Exhibition of Naples, Signor Siciliano, of Palermo, exhibited an arrangement for working the rudder of a ship by means of electro-magnets. The currents which actuate the electro-magnets are under the control of the ship's compass, any deviation of the compass from an assigned direction completes an electric circuit, which in turn, through the electro-magnets, acts on the rudder. Thus the compass and rudder form a perfectly automatic arrangement.

## MEDICAL.

*Volumes of the Cavities of the Heart.*—Professor the Rev. Samuel Houghton, F.R.S., in his recent lecture (June 24) before the Royal Institution, attempted to compute the volumes of the ventricles of the heart. Admitting the principle of least action, he said: "I can predict a thing that at first sight appears very strange. I can find the ratio which the volumes of the two cavities bear to each other by the measurement of the lengths of the fibres that surround them. On measuring these fibres it comes simply to this. Let L be the length of the fibres that go round the entire heart: let *l* be the length of the fibres that go round the left ventricle. Find those lengths and cube them. The ratio of those cubes will be proportional to the sum of the right and left ventricles divided by the left. There are theoretical grounds which I believe are almost of themselves sufficient to entitle us to believe that these two cavities are of equal volume, and therefore that this fraction will come out equal to 2. I have taken, however, a more certain mode of determining this by collecting together all the obser-

vations of direct measurement of these volumes that I can find, and I find that the mean is 2·125. From theoretical grounds, I believe that more accurate experiments and observations will prove that the decimal fraction of an eighth must be struck off, and that the true proportion is represented by 2. Certainly 2 is the number given by the most accurate of the ten observers. But now to my verifications. I measured the length of the common fibres in the hearts of a great number of oxen, and I find it to be 10·875 inches. I measured the length of the fibres that go round the left ventricles in the same hearts, and I find as the mean of many measurements 8·625. Well, I suppose there is no one present here who is a good enough arithmetician to tell me at sight what the ratio of the cubes of those numbers would be. I have cubed the numbers, and their ratio comes out 2·004. I believe that to be a remarkable result, and to entitle us to assert that the principle of least action applied to the problem of the heart, is capable of solving it a step beyond what it has been solved, and bringing us within reach of the knowledge of one more of the wonderful laws the Creator. How it would rejoice the soul of the great Kepler if he had known that the ratio of the length of the fibres in his own heart was in the proportion of cube root of 2 to 1!. Divine Geometry! Queen and mistress of philosophy, thy right to rule the sciences shall never be disputed!"—*British Medical Journal* for June.

*Dynamics of Nerve and Muscle.*—From the "British Medical Journal" (which contained an able review on it) we learn that Dr. Radcliffe has published his views on this subject. We have not seen the book, however, which is published by Messrs. Macmillan. Hence we can do no more than announce the fact of its publication. It is to be regretted that Messrs. Macmillan do not send out their books more freely for review. We have had to write for at least three or four of their recently published works. We have no care for the interests of a publisher, but we do not consider that an author is fairly served by the negligence of his publisher in regard to sending out his book for review. A lesson might well be taken from the American publishers in this respect.

*Dr. Lionel Beale a "Baly" Medallist.*—The "Baly Medal" of the Royal College of Physicians, London, has been awarded to Dr. Lionel Beale, and was presented to him immediately after the termination of the Harveian Oration.

*Death of Von Niemeyer.*—We regret to notice the decease of the distinguished Dr. Felix von Niemeyer, author of the well-known work on the Practice of Medicine, which has met with such a remarkable success in England and America through the translation of Drs. Humphreys and Hackley. We have no particulars of Niemeyer's case, except that he had recently returned from France, where he had been engaged in studying typhus and dysentery in the army. At the time of his return he was sick, but his illness was not believed then to be of a serious character.

*Todd and Bowman's Physiology.*—The second part of the new edition by Dr. Beale, F.R.S., has made its appearance. It is devoted to the consideration of fibrous, elastic, and connective tissues, cartilage, bone, and fat. It is of course a very valuable work, and we wish it were completed by the present editor, who is certainly somewhat slow at his work.

## METALLURGY, MINERALOGY, AND MINING.

*The Manufacture of Steel.*—Mr. David Forbes' usual quarterly report on the mining and metallurgical resources of the several States of Europe, gives us most valuable information. In fact, it considerably lightens our task, for it gives in a condensed and thoroughly comprehensive form, everything that has been done for the quarter. We find it stated that M. Aristide Berard has recently introduced into practical operation, at Givors, in France, a process for the direct conversion of pig-iron into steel, for which, among other advantages, he claims that it effects a partial purification of the iron, by eliminating the sulphur, phosphorus, arsenic, &c.; at least, to such an extent, that commoner brands of pig-iron, which by no process at present known could be used, may be employed for making steel suitable for the manufacture of rails, tyres, &c.; and that, by the combined action of air and gas, alternate oxidising or reducing effects may be obtained at pleasure, so that the decarbonisation or recarbonisation, and consequent uniform nature of the product may be regulated, whilst at the same time the waste is reduced to a minimum. The main features of the process are—the conversion of the fuel employed into a gaseous state, the use of a jet of superheated steam in so doing, and the employment of a peculiarly-shaped converting furnace, in which, from three to five tons of cast-iron is treated at a time, the charge being run into the moveable bed of the furnace, in the molten state, direct from the blast furnace or cupola. Spiegeleisen is added in the operation, and the waste is stated to be not more than from seven to eight per cent., whilst the operation is said to require only from one hour to one hour and a half. The process has been fully described in a pamphlet, published by M. Berard.

*Anthracite Coal Trade of Pennsylvania.*—Mr. Peter W. Sheafer, a well-known engineer at Pottsville, Pa., has prepared a diagram, which is published in "Silliman's American Journal," exhibiting the progressive development of the anthracite coal trade of Pennsylvania. It embraces the period of fifty years, from 1820 to 1870, and an accompanying table gives in detail, for each year, the yield of anthracite of the four great subdivisions of the anthracite region, the Lehigh, the Schuylkill, the Wyoming, and the Lykens Valley, Shamokin, &c. We take the export in tons, for the years below specified, from the table:

| | Lehigh. | Schuylkill. | Wyoming. | Lykens V., &c. | Total |
|---|---|---|---|---|---|
| 1820 | 365 | | | | 365 |
| 1830 | 41,750 | 89,934 | 43,000 | | 174,734 |
| 1840 | 275,313 | 475,091 | 148,470 | 15,505 | 864,384 |
| 1850 | 690,456 | 1,782,936 | 827,823 | 57,684 | 3,358,899 |
| 1860 | 1,821,674 | 3,270,516 | 2,941,817 | 479,116 | 8,513,123 |
| 1870 | 3,172,916 | 3,853,016 | 7,825,128 | 998,839 | 15,849,899 |

The Schuylkill trade began in 1822, with the exportation of 1,480 tons; the Wyoming, now twice the largest, in 1829, with 7,000 tons; the Lykens Valley, &c., in 1839, with 11,930 tons.

*Puddling by Siemens' Gas Furnace.*—Mr. Forbes' Report states that in the

"Preussischer Zeitschr. f. Berg. Huetten u. Salinenwesen," 1870, p. 145, will be found an extremely interesting paper by Dr. Kosmann, in which he gives the results of a comparison between the effects and relative economy of puddling in the ordinary manner, and when done by Siemens' regenerative gas puddling furnace; although short, the space at command will only allow of our giving the conclusions arrived at, which are (1), that the Siemens' furnace is to be preferred in all cases where an extremely high heat is required, and where the fuel is of bad quality and unsuited for producing sufficient heat when consumed in the ordinary way; (2) whenever a fixed temperature or a certain quality of flame is required for any length of time; (3) when no use of the waste heat of the flame (as for heating steam boilers to drive machinery) is required. And, in addition, there is less waste and also somewhat less loss of iron in the slag with the Siemens than with the ordinary furnace, as may be seen by a comparison of the chemical analysis of the respective slags.

| | Ordinary furnace | Siemens furnace |
|---|---|---|
| Silica | 11·98 | 15·36 |
| Alumina | 1·11 | 1 18 |
| Protoxide of Iron | 68 69 | 66·33 |
| do. Manganese | 1·00 | 0·92 |
| Lime | 1·79 | 2 51 |
| Magnesia | 0 24 | 0·92 |
| Soda and Potash | 2·13 | 0 72 |
| Phosphoric Acid | 14·43 | 14·28 |
| Sulphur | 0·24 | 0·28 |
| | 101·61 | 102·50 |

The amount of phosphorus or sulphur eliminated in the slag is about the same in both instances. If, however, the fuel is of good quality, and the waste heat is employed for raising steam, then there appears to be little, if any, advantage in the employment of the Siemens furnace, which is known to be extremely costly, both in original construction and in subsequent repairs.

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## MICROSCOPY.

*Sacs in the Tibia of a Flea.*—A very long and somewhat important paper has appeared in the Quekett Club "Journal" for July, on the above subject. The function of these sacs, in the author's opinion, is somewhat peculiar. He says that the action of the contractile sac of the upper tarsal joint is first, by slow distension, to become filled with air, the membranous sac of the tibia simultaneously collapsing. When fully distended, the tarsal sac suddenly contracts to about one-fourth its previous diameter, when at the same moment the membranous sac of the tibia becomes fully inflated. This rhythmical, alternate movement sometimes proceeds, regularly, at the rate of two or three pulsations in the minute, but this is not always the case, as he has frequently found that it is suspended for longer or shorter periods, and in many specimens it is altogether wanting. Believing that

these remarkable organs have not hitherto been observed, he has devoted much attention to them, and he thinks he is justified in expressing the opinion that they probably serve a very important and hitherto unsuspected purpose, in the respiratory system of the animal, and further, if he is right in his conjecture, that similar organs will probably be found to exist in many other insects. He thinks it possible, then, that these contractile sacs serve the purpose of pumps or syringes, by means of which air is drawn through the external orifices or spiracles, and propelled through the minute capillary vessels of the tracheal system.

*An Immersion Paraboloid* is described by Mr. B. D. Jackson in the "Journal of the Quekett Club" for July. It is formed of a solid paraboloid of glass, ground to a different curve than the dry form, and instead of having its emergent surface hemispherically hollowed out, it is left nearly flat, a very slight concavity only being given. This concavity is so slight as to be hardly perceptible, but it is intended to permit the slide in contact with it, by means of the water film, to be moved to and fro without danger of scratching the glass top of the illuminator—no very difficult thing to do, in spite of the apparent hardness of the substance. The stop to prevent direct rays passing into the microscope is cemented to the lower surface of the paraboloid. The object (*Eupodiscus argus*) is shown by a quarter-inch binocular with a black field; the angle of the object-glass being about 110°, a result he has not been able to attain so satisfactorily by any means previously employed. There is no loss of light by reflection from the lower surface of the glass, since the rays pass almost in straight lines from the curved sides to the focus. The ordinary test diatom slide, when mounted dry on the cover, as usual, presents a curious appearance, the field being dark, with a small spot of orange-brown light, occupying about one-fifth of the diameter, the spherules, however, being shown distinctly. He has not been able as yet to use this illuminator with higher powers, the fog surrounding the object unpleasantly.

*The French Erecting Prism is a Camera Lucida.*—The July number of the "Monthly Microscopical Journal" contains a letter, in which the writer asserts most positively that this prism answers the purpose of a camera lucida.

*The Degeeria Domestica, or Speckled Podura,* is, according to Mr. Wenham ("Monthly Microscopical Journal," July), when shown opaquely under a $\frac{1}{12}$th or upwards, a specially beautiful object. The scales are apparently much thicker than in other species, and the ribbings or ! ! ! markings are of a reddish-brown colour—not beaded, but slightly constricted at regular intervals, like the short antennæ of some insects, and in the deep intercostal spaces there are numerous thin septæ, or transverse bars, very fine and distinct, of a greyish tint. Both these and the slightly "varicose" spaces on the ribs may be displayed in the form of beads, by dodging the illumination. Where practicable, some form of opaque illumination should always be employed for verifying the structure of these objects, for we are in this case quite free from the errors of diffraction, which more or less accompany objects seen by transmitted light, and cause an indistinctness of outline.

*The "Wolf-rock" under the Microscope.*—Mr. S. Allport, in the "Monthly Microscopical Journal," says that, when examined by the eye or simple lens,

the rock is seen to consist of a yellowish-grey compact base, in which crystals of clear glassy felspar are imbedded; they exhibit no striæ; their fracture is sharp and splintery. A thin section examined in polarised light with crossed prisms exhibits a beautiful group of crystals of felspar and nepheline porphyritically imbedded in a fine-grained matrix composed of minute crystals of nepheline, felspar, and hornblende; when cut very thin, the hornblende alone exhibits colours, the hexagonal sections of nepheline being black, the rectangular white; the felspar is also either dark or light, and the general appearance is that of a mosaic of dark and light stones interspersed with small brilliant-coloured crystals of hornblende; the whole forming a matrix in which the larger crystals are set. In thicker sections the felspar and nepheline display fine colours, but the minute structure is not so well seen.

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## PHYSICS.

*A New Key for the Morse Printing Telegraph* was described to the British Association at Edinburgh by Professor Zenger. He said that, at the Norwich meeting, he had had the honour to show a new automatical key for the Morse printing telegraph, which printed three different signs—viz., a point, a short line, and a longer line. That arrangement restricted the telegraphist to a certain speed, but he had constructed the key now shown in order to allow a clever telegraphist to obtain the highest speed attainable. He showed a model, and explained it in detail. The rate of velocity was indicated by a small bell sounding as often as the cylinder revolved. By the mechanical arrangement, whatever the speed of the paper and the clockwork moving it, the relative length remains unalterably the same. On the working instrument, the printing apparatus with its rollers for the paper sheet is attached to the key, forming only one apparatus together. By using three signs, the combination of 1, 2, and 3 elements gives 39 signs. This will do for all letters, figures, and phrases commonly used, and spare nearly 30 degrees of space, and therefore of time of transmission. In a brief discussion, it was said that the adoption of Professor Zenger's apparatus would do away with the mistakes now so often made by the doubting whether the short line meant a line or a point.

*The late Professor Payen.*—Physics has lost one of its great masters through the death of Payen, which occurred a few months since. "Les Mondes" of June 29 contains a short account of his life. In 1824 he was the first who pointed out the rational method of applying manures in agriculture, and about the same time brought forward the theory of the decolouration of liquids by animal charcoal, also suggesting the use of the residues of sugar-refining (scums which contain more or less animal charcoal along with other substances) in agriculture. In 1830 he laid the foundation of the proper valuation of manures, according to their richness in nitrogen. The late author's exhaustive researches on amylaceous matter settled the exact structure, mode of formation, and the derivation of dextrine and glucose from starch; the existence of diastase, also, was first pointed out by the deceased. Prof. Payen, very well known, also, among a

great many others, by his excellent work on industrial chemistry, "Précis de Chimie Industrielle" (Paris: Hachette & Co, 1867), was, since 1842, a member of the French Academy of Sciences, and held the professorships of Industrial Chemistry at the École Centrale des Arts et des Manufactures since 1830, and at the Conservatoire des Arts et Métiers since 1839. The deceased was not only a very eminent scientific man, but was thoroughly and practically acquainted with almost every branch of industrial pursuits. Anselme Payen was born at Paris on January 17, 1795, and was in early life first the technical manager of beetroot sugar-works at Vaugirard, and afterwards of very large chemical works near Paris.

*A New Form of Steam-blast.*—A paper on a new form of steam-blast was communicated to the British Association at Edinburgh, by Mr. W. Siemens, F.R.S. The new blast is employed for the movement of air in the pneumatic tubes connected with the central telegraph station in London. It is said to cost only 40*l.*, and will do the same work as an engine which costs 2,000*l.*

*A New Form of Galvanometer* has been devised by Mr. John Trowbridge, Assistant Professor of Physics in Harvard College (U.S.). It is described at length in "Silliman's American Journal" for August. It would be impossible to describe it without the cuts which accompany the article. We merely refer to it because it may interest some of our readers.

*Transport of Salts by Electrical Discharge.*—This paper, which is by M. Becquerel, appears in the "Comptes Rendus" for June 26. His essay treats on some phenomena observed by the author while experimenting on the effect of only moderately strong electrical discharges when certain chemical compounds are placed in the route of the electric current. As results from these experiments, the author finds that the undermentioned salts and other chemicals are transported by electrical discharges in the direction from the negative electrode to the positive electrode, but not again the reverse way. These salts and substances are ferricyanide of potassium, bichromate of potassa, chloride of barium, chloride of sodium, chloride of potassium, sulphuric acid, phosphoric acid, chloride of ammonium, and protochloride of iron. The following are among the substances which are not transported by any electric current, whatever its direction:—Chloride of cobalt, chloride of platinum, nitrate of silver, caustic potassa, and sulphate of potassa.

*A Meteorological Observatory in the Azores.*—Dr. Buys-Ballot, after briefly pointing out the great importance, not simply in a scientific, but also in a mercantile and industrial point of view, of having, on one of the islands just named (they belong to Portugal, and are situated at about 40° N. lat., and 30° W. of Greenwich), a meteorological station, connected by submarine telegraphs with Europe and America. The author states that, probably, by September 1872, this desirable object will be accomplished by the activity of M. Fradesso da Silveira, the Director of the Observatory at Lisbon.—*Comptes Rendus*, June 12.

*Low Temperature of the* 18*th of May and the First Days of June.*—M. Sainte-Claire Deville gives a lengthy paper ("Comptes Rendus," June 19) containing a series of communications received by him from different localities in France. It appears that, even in the very south of that country, the temperature fell during this period to so low a degree as

has not been witnessed within man's memory at this time of the season; moreover, heavy storms, accompanied by deluging rains and seriously destructive hail, have occurred in many parts of France; while near Paris, at St. Germain en Laye, he found the temperature was $-3{\cdot}5^\circ$ on May 18, in the morning, at a height of 33 centimetres above the soil (a meadow).

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## ZOOLOGY AND COMPARATIVE ANATOMY.

*New North American Phyllopoda.*—A series of notices of several new species appears from the pen of A. J. Packard, M.D., in "Silliman's American Journal" for August and the earlier months. We merely call attention to them because, of course, it would be out of our power to reproduce them here.

*A Grand Dredging Exploration.*—Professor Agassiz has accepted an invitation extended to him by the American Coast Survey Bureau to take passage on the iron coast-survey steamer, which has recently been built at Wilmington, Delaware, and which was to sail for the Pacific coast in September. The expedition will take deep-sea soundings all the way, and extensive collections of specimens will be made for the Museum of Comparative Zoology at Cambridge. Secretary Boutwell has written to the Secretaries of State and the Navy, asking that naval and other officers may be instructed to afford such courtesy and assistance to the exploring party as may be desirable. We learn also that Count Pourtales, of the Coast Survey, and Dr. Hill accompany the expedition.

*The Brachiopoda obtained by the United States Coast Survey Expedition.*—This expedition was in charge of L. F. ae Pourtales. The report is published in the Bulletin of the Museum of Comparative Anatomy (vol. iii. No. 1), and is by Mr. W. H. Dale. In this paper all the species dredged by Count Pourtales are fully described, and the synonymy of these and other species and genera is well worked out. The anatomy of several of the species is described at considerable length. Two lithographic plates, chiefly anatomical, illustrate this paper.

*The Development of a Giant Gregarine.*—The anatomy and development of *G. gigantea*, an enormously large species, has been worked out by M. E. Van Beneden, who has published a long and important memoir, accompanied by a plate, on the subject. He has found in the lobster's intestine multitudes of small protoplasmic masses resembling the *Protamœba primitiva* of Haeckel, with certain distinctions from it, however. They are distinguished from the true Amœba by the absence of a nucleus and a contractile vacuole. These have no projections from them. There are, however, others which have one, or more frequently two, prolongations in the form of arms, which M. Van Beneden says resemble the mobile stem of Noctiluca, and these forms he calls generating *cytodes*. He then describes the movements of them, one of the arms or projections of which is motionless. After a time the other increases in length till at last it breaks away, and having specially an undulating motion, it is like a nematoid worm. Curiously enough, when

the moveable arm has been discharged, the further development of the arm at rest begins, and it goes through the same process of development and motion as the preceding, with this difference, that instead of becoming detached from the central mass, it gradually absorbs it as a vertebrate embryo absorbs the contents of the vitelline sac. The resemblance of the animal thus formed to the Nematodes has led the author to style them *pseudofilariæ*. He then proceeds to describe the further development of those peculiar bodies into *Gregarina gigantea*. Several other subjects of kindred interest are discussed in the memoir, which is both full and complete.

*The Meteoric Origin of Life.*—This new hypothesis, which was originated by the President of the British Association in his Edinburgh address, is somewhat startling. The notion that life first came upon the globe in a glistening fire-ball, which brought some fragments of moss along with it, is a curious notion enough; but it by no means gets rid of the difficulty of the origin of life; it merely carries it back a little further.

*Zoological Stations.*—Dr. Sclater, F.R.S., read before the British Association (at Edinburgh) a report from the committee for the formation of zoological stations in different parts of the globe. It stated that an observatory at Naples had been arranged for, and drew attention to the importance of establishing a zoological station in the British Islands, and to the opportunity now afforded for such a proposition by the cessation of the grant to the Kew observatory. Until a recent date, it was submitted, the Association had given considerable sums for dredging explorations, but in consequence of the advance of zoological science the problems were much changed, and their nature was such as to demand the assistance of the Association in other directions. The careful study of the development and habits of marine animals could only be carried on by the aid of large aquaria and cumbrous apparatus, which an individual could hardly provide for himself. This, and the copious supply of animals for observation, could be provided by such a co-operative institution. A resolution was accordingly submitted, to the effect that the committee of Section D recommend that a committee of the Association be formed for the purpose of erecting a zoological station at a convenient place on the south coast of England—say Torquay—and that a sufficient sum of money—say 500*l.*—be placed at their disposal, either by a single or by a series of annual grants. After reading the report, Dr. Sclater remarked that as there were three or four aquaria already established in this country, he thought the best plan would be to establish one in an entirely different part of the world; as, for instance, under the tropics, where animal life was entirely different.

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END OF VOL. X.

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